This is a digital copy of a book that was preserved for generations on library shelves before it was carefully scanned by Google as part of a project to make the world's books discoverable online.

It has survived long enough for the copyright to expire and the book to enter the public domain. A public domain book is one that was never subject to copyright or whose legal copyright term has expired. Whether a book is in the public domain may vary country to country. Public domain books are our gateways to the past, representing a wealth of history, culture and knowledge that's often difficult to discover.

Marks, notations and other marginalia present in the original volume will appear in this file - a reminder of this book's long journey from the publisher to a library and finally to you.

Usage guidelines

Google is proud to partner with libraries to digitize public domain materials and make them widely accessible. Public domain books belong to the public and we are merely their custodians. Nevertheless, this work is expensive, so in order to keep providing this resource, we have taken steps to prevent abuse by commercial parties, including placing technical restrictions on automated querying.

We also ask that you:

+ *Make non-commercial use of the files* We designed Google Book Search for use by individuals, and we request that you use these files for personal, non-commercial purposes.

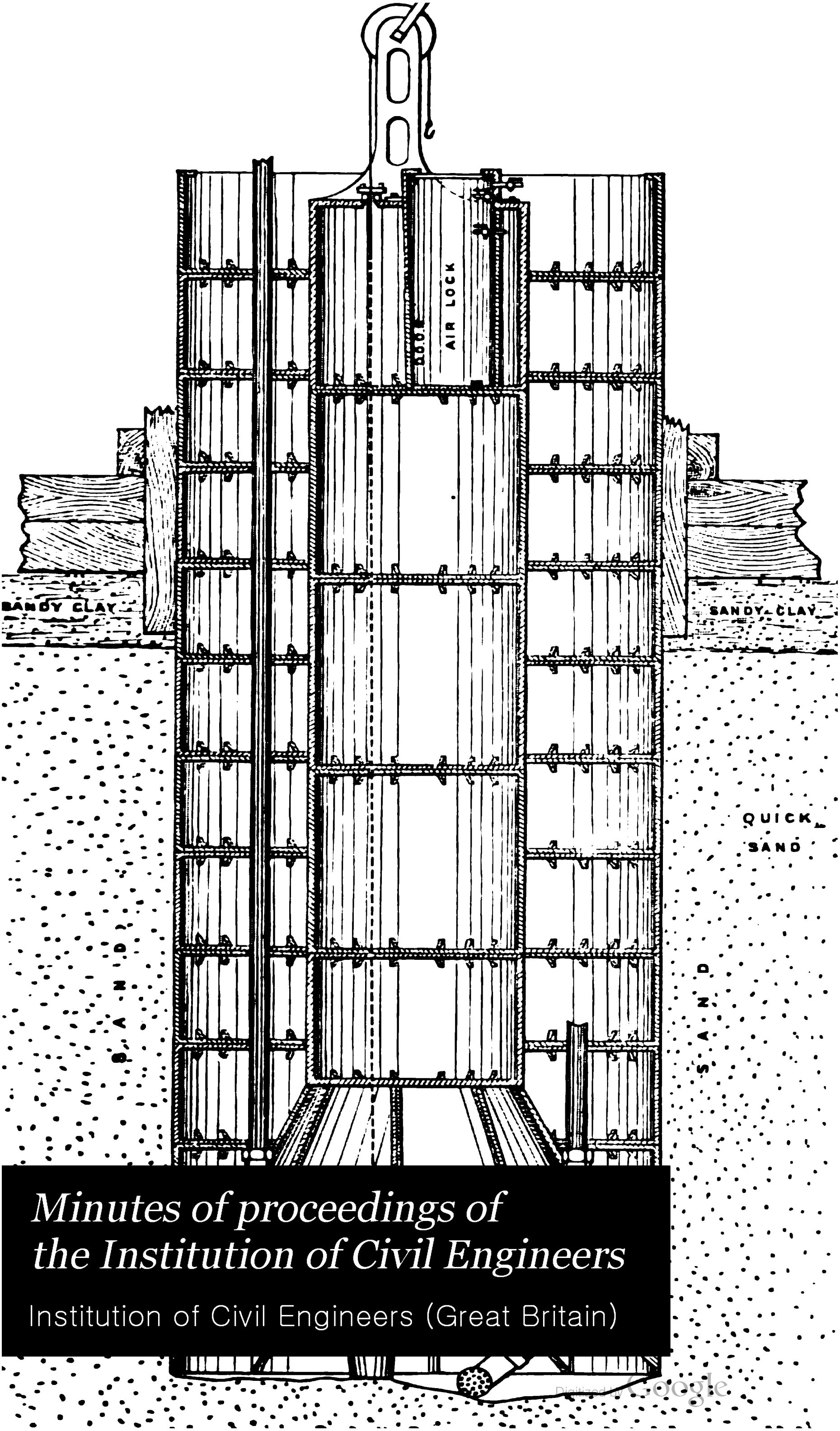
+ *Refrain from automated querying* Do not send automated queries of any sort to Google's system: If you are conducting research on machine translation, optical character recognition or other areas where access to a large amount of text is helpful, please contact us. We encourage the use of public domain materials for these purposes and may be able to help.

+ *Maintain attribution* The Google "watermark" you see on each file is essential for informing people about this project and helping them find additional materials through Google Book Search. Please do not remove it.

+ *Keep it legal* Whatever your use, remember that you are responsible for ensuring that what you are doing is legal. Do not assume that just because we believe a book is in the public domain for users in the United States, that the work is also in the public domain for users in other countries. Whether a book is still in copyright varies from country to country, and we can't offer guidance on whether any specific use of any specific book is allowed. Please do not assume that a book's appearance in Google Book Search means it can be used in any manner anywhere in the world. Copyright infringement liability can be quite severe.

About Google Book Search

Google's mission is to organize the world's information and to make it universally accessible and useful. Google Book Search helps readers discover the world's books while helping authors and publishers reach new audiences. You can search through the full text of this book on the web at `http://books.google.com/`



Minutes of proceedings of the Institution of Civil Engineers

Institution of Civil Engineers (Great Britain)

MINUTES OF PROCEEDINGS

OF

THE INSTITUTION

OF

CIVIL ENGINEERS;

WITH OTHER

SELECTED AND ABSTRACTED PAPERS.

VOL. LXXI.

EDITED BY

JAMES FORREST, ASSOC. INST. C.E., SECRETARY.

LONDON:
Published by the Institution,
25, GREAT GEORGE STREET, WESTMINSTER, S.W.
1883.

ADVERTISEMENT.

The Institution as a body is not responsible for the facts and opinions advanced in the following pages.

LONDON: PRINTED BY WM. CLOWES AND SONS, LIMITED, STAMFORD STREET AND CHARING CROSS.

[redacted] COOLEY
J-13-'03

CONTENTS.

Sect. I.—MINUTES OF PROCEEDINGS.

14 and 21 *November*, 1882.

28 *November*, 1882.

5 and 12 *December*, 1882.

19 *December*, 1882.

SECT. II.—OTHER SELECTED PAPERS.

SECT. III.—ABSTRACTS OF PAPERS IN FOREIGN TRANSACTIONS AND PERIODICALS.

ERRATA.

Vol. lxx., p. 400, Table II., second col., line 8. In some of the copies a figure "2" is missing between "0 0 rise."
" " " 444, line 5, for "lith" read "lyth."
" " " " " 7, for "Liège" read "Liége."

THE
INSTITUTION
OF
CIVIL ENGINEERS.

SESSION 1882-83—PART I.

SECT. I.—MINUTES OF PROCEEDINGS.

14 November, 1882.

SIR W. G. ARMSTRONG, C.B., F.R.S., President,
in the Chair.

(*Paper No. 1876.*)

"Recent Hydraulic Experiments."

By Major ALLAN CUNNINGHAM,[1] R.E., Fell. of King's Coll., London.

I. INTRODUCTION.

THIS Paper is for the most part a short general account of some extensive experiments on the flow of water in the Ganges Canal in Northern India, lasting over four years (1874–79), of which a detailed report was published in 1881.[2] All experimental and argumentative details are here necessarily omitted.

The main object of the undertaking was to interpolate something between Mr. Bazin's experiments on small canals and the experiments on American rivers, chiefly with a view to discharge-measurement on large canals, the proper measurement of such discharge being of great practical importance, but hitherto attended with much uncertainty. For any such work there are good opportunities in India from its system of canals both large and small, pre-eminent among which is the Ganges Canal.

The extensive scale of the operations can be judged from the following abstract:—

565 Sets of vertical velocity-curves, each set containing velocities measured thrice at every foot below the surface.

---

[1] The Author has since been elected an Assoc. Inst. C.E.

[2] "Roorkee Hydraulic Experiments." By Capt. Allan Cunningham, R.E. Roorkee, 1881. This Paper is divided into sections bearing the same numbers and titles as the chapters of the "Roorkee Hydraulic Experiments."

543 Rod-velocities taken with above, each measured six times.
133 Sets of surface, mid-depth, and bed velocity-curve work, each set containing velocities measured thrice at from twelve to seventeen points on the transversal.
581 Sets of mean velocity-curves, each set containing velocities measured thrice at from ten to twenty-one points.
313 Central surface velocities, each measured forty-eight times.
440 Surface-slopes (about 150 taken on both banks).
90 Silt-collections.
40 Evaporation measurements.

The total number of velocity-measurements was thus about 50,000. Besides these there were many occasional special experiments which together form an important addition.

Nearly all the observations, various, or even unpractical as some of them may seem, were strictly subordinate to the great practical end in view—viz. discharge-measurement: thus the first two items above were the earliest steps directed, at the suggestion of the Mississippi experimenters,[1] to discover some rapid means of measuring the mean velocity past a vertical, and actually led to showing that the tube-rod is well suited for this.

An important feature in this work is the great range of conditions and data, and therefore of results obtained, this being essential to the discovery of the laws of complex motion. Thus the velocity-work was done at thirteen sites differing much in nature, some being of brick, some of earth; in figure, some being rectangular, some trapezoidal; and in size, the surface-breadth varying from 193 feet to 13 feet, and the central depth from 11 feet to 8 inches. At one of the sites the ranges of some of the conditions and results were: central depth, from 10 feet to 8 inches; surface-slope, from 480 to 24 per million; velocity, from 7·7 feet to 0·6 foot per second; cubic discharge, from 7,364 to 114 cubic feet per second.

II. History.

This undertaking was initiated and planned throughout by the Author: the fieldwork (lasting from 1874 to 1879), reduction (finished in 1880), and publication (1880–81) were all carried out under his personal superintendence. Every precaution which seemed possible was taken to secure accuracy, of which full explanation is given in the detailed report. The experiments and reductions were made for, and at the expense of, the Indian Government, at a total cost of about £5,000, including publication. This may seem a large sum, but such work is necessarily expensive from a cause to be explained hereafter (Section VI.).

---

[1] "Report upon the Physics and Hydraulics of the Mississippi River," p. 292.

III. SITES.

The Ganges Canal abounds in long, straight, fairly uniform reaches, 200 feet wide and under. It has the peculiarity of being laid out in reaches of several miles in length, with masonry falls of about 8 feet drop at the tail of each. The original bed-slope was found to give too high a velocity, and to cause injury to the banks and bed. To meet this the falls were built up several feet after 1863, so that they are now all obstructed falls.

Although these raised crests must have diminished the velocity, the longitudinal sections of the reaches show that there has not been much silting up above them. As the canal is often full of silt, this shows that the water is in pretty rapid motion close to the actual bed, and disproves the idea, sometimes advanced, that an obstruction across a channel causes a stillwater pool above it roughly flush with its crest. There are means of temporarily raising still further the crests of the falls, thereby affording great control over the velocity and depth at any site due to a given quantity of water admitted into the reach, so that the mere gauge-reading at any site is no indication of the velocity or discharge through it. The great range of data and results obtained resulted from the exercise of this control.

The systematic observations were made at seven sites of different kinds—viz. at four sites in earthen channels of trapezoidal section, with surface-breadths of 186 feet, 189 feet, 193 feet, and 66 feet and under, and with central depths of 11 feet, 10 feet, 9 feet, and 6 feet and under respectively; also at one site of quasi-trapezoidal section, with a bed of clay and boulders, and banks consisting of flights of masonry steps, with a surface-breadth of from 171 feet to 150 feet, and central depth of from 11 feet to 1½ foot; also at a pair of sites in two similar rectangular masonry channels side by side, each 932 feet long, and 82 feet broad, with a depth of from 10 feet to 8 inches. These three last named are situate in the famous Soláni embankment 2¾ miles long, and Soláni aqueduct 1,112 feet long, and are extremely favourable for experiment.

Some minor measurements were effected at six other sites—viz. at two large sites in the Soláni embankment similar to the above, and also at four small sites in small distributaries of trapezoidal section in earth, with surface-breadths of 25 feet, 14 feet, 14 feet, and 13 feet and under, and with central depths of 4 feet, 3½ feet, 2 feet, and 3½ feet and under.

IV. VELOCITY.

A short discussion on the use of floats will now be given; the following short terms are used:—

Float.—Any freely floating instrument for measuring velocity.

Run.—The measured length through which a float is timed.

Float-course.—The intended course of a float within the run.

In fair course.—Close to the laid-out float-course.

Forward[1] *velocity.*—The resolved part of the actual velocity at any point, taken parallel to the current-axis.

Note that the forward[1] velocity is the only velocity of much use in practical hydraulics, and is therefore the quantity really sought in most practical velocity-measurement; hence also the single word velocity is commonly used in hydraulics in the limited sense of forward-velocity, and (for shortness' sake) will be so used in this Paper; the context will show when actual velocity is meant.

Objection has been taken[2] to the use of floats, that they do not measure velocity at all, and also that they move quicker[3] on the whole than the fluid particles about them. These objections are first met in the "Roorkee Hydraulic Experiments" by an argument showing that, in spite of the ever-varying and confused motion of the water, and of the consequent irregularity of the paths of floats, nevertheless "very small floats do measure fairly an average of the forward-velocities of the fluid particles successively in contact with them throughout their run"; and this measure (styled for shortness a float-velocity) must—for want of better means—be accepted as a measure of the forward-velocity at the middle of the float-course.

The following appear to be the true criteria of a good float-observation:—viz., that the float should be moving in relative equilibrium with the fluid just before entering the run; and that it should pass through the run in fair course and in the same state of relative equilibrium. A float-observation satisfying these conditions should be accepted as good, without any reference to its timing proving longer or shorter than others. Great stress is laid on this, as the practice of some persons is to select from a number

---

[1] A useful term adopted from Prof. James Thomson. "Proceedings of the Royal Society of London," vol. xxviii., 1878, p. 115.

[2] "Annual Report of the Chief of Engineers," U.S.A., 1869, p. 563.

[3] Dubuat's "Principes d'Hydraulique," 1816, vol. i., art. 220; Belidor's "Architecture Hydraulique," 1819, vol. i., p. 358; Weisbach's "Mechanics of Machinery, &c.," 1847, vol. i., art. 376; Prof. James Thomson. "Proceedings of the Royal Society of London," vol. xxviii., 1878, p. 124.

of observations those which agree nearly in timing, and to reject the rest; this practice appears wrong in principle. Next, no float which does not satisfy the above should be recorded; this saves useless records in the field-book.

In observing, the following precautions are important:—the ropes defining the run should be strained at the lowest possible level, and the pendants defining the float-courses should graze the water. The run should be the shortest compatible with accurate timing; and, in using short runs accuracy is essential both in laying out the run and in timing. Of these precautions the first and last are essential. The second is of great practical importance in saving time, for in consequence of the unsteady motion floats will seldom move throughout a long path in sufficiently fair course to be worth recording. The following experiment was tried: forty-eight surface-floats and forty-five rods of various lengths were timed, each one through four adjacent runs of 25 feet, 50 feet, 25 feet, and 100 feet. The results from the 50 feet and 100 feet runs were so nearly alike that the former was adopted as the standard run. Shorter runs of 25 feet and 12½ feet were used only close to the banks, where the irregular motion causes undue waste of time with a longer run.

The following advantages are claimed for floats: 1st, they interfere little with the natural motion of the water; 2nd, they measure velocity directly; 3rd, they can be used in streams of any size; 4th, they are not much affected by silt or floating weeds, &c.; 5th, they measure "forward-velocity"; 6th, they can be made up and repaired by common workmen; and 7th, they are very cheap. Fixed instruments fail in all these points. These reasons were held to justify the exclusive use of floats for all systematic velocity-work at Roorkee. The floats used were of three kinds: namely, surface-floats for surface-velocity; double-floats for sub-surface velocity; and loaded rods for mean velocity past a vertical. The surface-floats consisted either of a 3 inches by 3 inches by ½ inch pine disk, or of a 1 inch by 1 inch by ⅛ inch cork disk; the other two instruments will be described hereafter.

V. DETAILS.

Some details about observing the water-level, depth, and wind will now be shortly described.

Water-level.—The water-surface is in a state of constant slight but rapid oscillation, so rapid that it is impossible to note any but the highest and lowest water-level. The practice here was to note the highest and lowest water-level occurring in about one-half minute, and accept the mean of these as the free water-level of the time. By comparing the level so given by an adjacent still-water gauge, it was found that the free water-level stood, as a rule, slightly higher (even as much as 0·07 foot) than the still water-level. The reverse occurred only six times in sixty-three trials. This is an interesting confirmation of the law known from theory,[1] that the pressure in running water is less than in still water. The difference is, as a rule, very small; still for accurate investigation the water-level should always be taken in the same way.

Again, to determine with precision small differences of water-level at different places, the surface-level must be taken, not only in the same manner, but also at the same time at each place. Great attention was paid to this. It was found that in calm air the free water-level of opposite banks differs only slightly; but that in a high cross-wind it stands markedly (even as much as 0·07 foot) higher on the lee shore. These conclusions are based on thirty-six trials in calm air and sixteen trials in a high cross-wind. Hence all depths depending on a gauge reading, and also all quantities such as discharges computed from them, are liable to slight over- or under-estimation in a high cross-wind, according as a gauge stands on the lee- or weather-shore.

In all experiments lasting over any time the mean of the water-levels taken as above at the beginning and the end was accepted as the mean water-level of the experiment.

Average Depths.—In all cases of irregular beds average depths were obtained by sounding along each float-course in six or eight places, so as to give six or eight cross-sections about 25 feet apart. The average of these gave an average cross-section, from which all wet borders, areas, and hydraulic mean depths were computed; also the mean of the depths along each float-course was always taken for the depth thereon. Some such procedure seems an essential in all irregular beds, as the velocities through any site depend on the bed just above and below the site as well as at the site. The great labour thereof is a drawback, but not a justification for omission.

Wind.—The direction and velocity of the wind were observed at the beginning and again at the end of every experiment. The mode of recording this in the printed tables is worth notice, from its conciseness and convenience. The direction of the wind rela-

---

[1] Lamb's Mathematical "Theory of the Motion of Fluids," 1879, art. 28, 29, 30.

tive to the current (and not its real cardinal direction) is alone of importance as affecting the motion of the water; the direction of the wind was therefore always entered relative to the current-axis taken as the working meridian or N.S. line. Thus a wind from up-stream is entered as N. (north), a cross-wind from the right bank as W. (west), and so on; one of variable direction is entered V. The velocity deduced from observing the number of seconds occupied by one revolution of the hand of an anemometer was entered in feet per second; a wind too light to move the anemometer was denoted as *l* (light). The mean of a number of such wind-data was found from their resultant by graphic construction, by the theorem of the polygon of forces, and dividing it by the number of data.

The wind-results can only be looked on as a rough indication of some cause of disturbance of the motion of the water, as there is no known way of making any quantitative allowance for it. It is questionable whether the wind-data obtained were the best for the purpose. The highest wind, and also the total, or mean wind during each experiment, are also important data.

VI. UNSTEADY MOTION.

One of the most important conclusions from modern experiments is that the motion of water, even when tranquil to the eye, is extremely unsteady, so that there is no definite velocity at any point; but the velocity varies everywhere largely from instant to instant. The evidence of this is purely experimental; the variability of direction and magnitude may be studied separately.

Variability of Direction.—In any tolerably clear silt-bearing stream the motion of the particles of silt will be seen to be most confused; some hurrying up, some down, some crossways, in apparently ever variable disorder. The paths of floating chips cross each other very irregularly. This transverse motion forms one great difficulty in the use of floats. Again, weeds, strings, &c., fixed at only one end, sway about irregularly; such motion is unfavourable to the use of current-meters. The inference is that "the stream-lines interlace irregularly from instant to instant in all directions."

Variability of Magnitude.—This is well seen in the use of floats. It is a common thing for floats passing in succession under a rope, say, in the order A, B, C, &c., at 2 inches or 3 inches intervals, and running nearly in one float-course, to pass under a parallel rope 50 feet lower down stream in a different order, say, B, A, C, &c.,

or C, B, A, &c. It was found in the Roorkee experiments that the range of velocities deduced from a number of similar floats run in rapid succession over nearly the same float-course was commonly 20 per cent. of the mean. In some of Harlacher's experiments[1] a current-meter was fitted with electric connections, so as to record every revolution; the variations amount to from 20 per cent. in surface velocities to 50 per cent. in bed velocities in a few seconds. These rapid changes are certainly not due to faulty experiment, but to the variation of the motion itself.

Extreme variability in both direction and rate (technically unsteadiness) must be accepted, then, as a fundamental property of the motion of water. It is analogous to the well-known unsteady motion of the wind, which is exemplified by the swaying of a wind-vane, and by the fluttering of a pennon. This conclusion has an important practical bearing on both theory and experiment. First, as to theory, many formulas and investigations are based on two hypotheses of parallel motion and of steady motion, neither of which states exist even approximately. Next, as to experiment, any single velocity-measurement is clearly an accidental value, possibly the maximum or minimum; non-synchronous velocity-measurements at different points are therefore commonly incomparable. Hence in most cases the average values of velocities are the only comparable ones, and therefore the only ones of much practical use. With current-meters this averaging can be done by letting them run for a considerable time. With floats it might be thought sufficient to increase the length of run; but this has been explained above to involve too great waste of time; it can then be done only by repeated measurement of each velocity sought. As to the number of repetitions necessary, twenty-seven instances are quoted in the Roorkee experiments of a velocity-measurement from twelve to one hundred times repeated, from which it appears that the means of twenty-five and of fifty repetitions do not differ more than 0·05 foot per second. The twenty-seven cases are of very varied kind; they include eighteen cases of central surface velocities, each forty-eight times measured in calm air at eight widely different sites; and nine cases of central velocity-measurements at 5, 6, and 9 feet depths, ascertained with various instruments, repeated from twelve to one hundred times. From this evidence it was concluded that a fair average might be expected from about fifty repetitions, and the number forty-eight was chosen as the standard to be aimed at. This is very laborious work;

---

[1] Minutes of Proceedings Inst. C.E., vol. lxvi., p. 37.

so many repetitions could seldom be done in less than half an hour, and they sometimes took an hour. Such experiment is necessarily tedious and expensive; as it would take many hours to obtain average values at only a few points.

Again, a serious practical difficulty arises in that the state of the water is itself very varying in a canal, chiefly from the regulation of the supply into, and withdrawal from, each reach; whereas, it is essential that observations intended to be combined should be made in nearly the same state of water. The system adopted, to secure this condition throughout a number of velocity-measurements at many points on any one vertical or on any one transversal, was to measure the velocity thrice at each point thereof, in turn from end to end of the vertical or transversal in question, as quickly as possible. The mean of each trio is accepted for permanent record. Such observations, together with all data collected with it, *e.g.*, gauge-readings, surface-slope, state of wind, &c., are briefly styled a "set." When one set was done, a second was undertaken, then a third, &c., each complete in itself, as long as the working hours, weather, and state of water permitted. Each set is thus a complete group of data collected within a short time, and therefore in as nearly the same state of water as practically attainable.

Afterwards all sets of similar character, namely upon the same vertical or the same transversal, at the same site, and nearly in the same state of water, were collected into groups shortly styled "series," and the means of the several data taken. The proper criterion of similarity of the state of water seems to be a close similarity of gauge-readings throughout the reach and of the control at the head and tail of the reach. The actual practice was, however, to combine all sets with nearly the same mean velocity and nearly the same water-level, a range of 0·3 foot of water-level being admitted. The irregularity of the wind forced the combinations to be made almost irrespective of wind. The fairness of these combinations in forming averages is a matter of great importance; in other experiments on a large scale such strict rules have not been observed, combinations having been used of work in very different states of water, and even of dissimilar work when judged as above.[1] The means of the several data in a series evidently form a set of mean data taken under nearly the

---

[1] "Report upon the Physics and Hydraulics of the Mississippi River," pp. 226, 227, 230, 232, 236; "On the Theory of the Flow of Water in Open Channels." By R. Gordon, 1875, pp. 18, 19; "Report of the Surveys and Examinations of the Connecticut River." By T. G. Ellis, 1878, p. 313.

same average conditions. The field work was, when possible, repeated so as to obtain about sixteen sets in a series; each mean of velocities would thus be the mean of 16 × 3 = 48 repetitions, and therefore a fair average value. But from canal exigencies certain states of water occurred very seldom; thus the series for such states contain only a few sets, some only one set; this was unavoidable. Except for illustrating special points, such as unsteady motion, only the mean results of series have been used in discussion.

On plotting the velocity-ordinates so as to form curves showing the forward velocities at all points of a vertical or of a transversal, to be styled for shortness vertical and transverse velocity-curves, it is evident that curves formed from a single set, or from only a few sets, are very irregular, and that they become more and more regular in outline with increase of the number of sets in the series represented. Curves formed from many sets, each ordinate of which is therefore an average velocity, may be styled average curves; these are the only ones from which geometric properties can be readily traced, and are the only ones worth discussing. They exhibit the following general properties:—

"They are very flat, and are mostly everywhere convex down stream (exceptions being traceable to irregularity of bed or banks)."

"The maximum velocity is furthest from the resisting margin."

Taking the evidence as a whole it would seem that, though the motion is very unsteady in detail, yet there is nevertheless an average steady motion.

VII. SURFACE-SLOPE.

One of the most important hydraulic data is the surface-slope of a stream; this, from its extreme smallness and from the oscillation of the water, is difficult to measure. Great care was taken to secure the best results; it must suffice to say here that the water-levels at the two points concerned in finding any one slope were always taken at the same time by signal; for shortness these will be called the slope-points, and the distance between them the slope-length.

First, it was found from twelve trials of slope-lengths of 2,000 feet and 4,000 feet symmetrically situate about the same site, that the deduced slopes are liable to differ by 25 per cent. This shows that surface-slope is probably a quantity not admitting of proper measurement, as different slope-lengths give such very different results. It seems clear that the slope-length should be the shortest, compatible with accuracy in measuring the surface-fall therein; also that to give comparable results a standard slope-length should be adopted, and that the same slope-points should always be used at any one site. The standard slope-length on this work was 2,000 feet.

Next, by taking thirty-one pairs of slope-measurements in pairs at the same time at three sites 1 mile to 2 miles apart, it was found that the surface-slope may be very different at different parts of the same reach.

Again, from one hundred and eighty-one pairs of slope-measurements on both banks at six sites, the measurements on the right bank being taken two or three hours after those on the left bank, it appears that the surface-slopes of opposite banks may differ 50 per cent. Hence it would seem that a surface-slope should always be deduced from simultaneous water-levels on both banks, two on each bank, thus requiring four skilled observers. This course could not be adopted from want of observers. The general conclusion from over five hundred cases was that surface-slope measurement is so delicate a matter that the results are of doubtful use.

From numerous data it was found that the surface-gradient at different parts of a reach depends partly on the depth, but much more on the control at the tail. Also, that the figure of the free surface along a reach depends chiefly on the control at the tail; thus, during high supply with constant obstruction at the tail, the free surface sinks in nearly parallel lines in the upper sub-reach, and in converging lines with decreasing gradient in the lower sub-reach, and therefore becomes a concave surface; and the concavity is greatly increased by increase of the obstruction at the tail.

VIII. SURFACE-CONVEXITY.

Since fluid pressure decreases with velocity, there is some ground for expecting[1] that the surface of a stream should be convex, *i.e.* stand highest about the middle or where the motion is quickest. The experimental evidence of this is very small.

In the atlas illustrative of Darcy and Bazin's hydraulic experiments there are forty-six carefully drawn cross-sections of

---

[1] "On the Steady Flow of a Liquid." By Henry Moseley, Canon of Bristol. Philosophical Magazine, vol. xlii., pp. 352, 353; vol. xliv., p. 44, and "Report upon the Physics and Hydraulics of the Mississippi River," pp. 195, 196, 303.

channels less than 6½ feet wide;[1] these include nine cases of central elevation, and eight of central depression above or below both banks, and twenty-nine doubtful cases. On the large scale the Author knows of only two cases in point. In the "Annales des Ponts et Chaussées" for 1848, Mr. Baumgarten states that once the surface of the Garonne was $\frac{4}{10}$ foot and $\frac{1}{10}$ foot above that at the banks when the river was rising 5 feet in a day, and was another time nearly plane when the river was falling 8 feet in a day.[2] Again, General F. H. Rundall states[3] that on the Godavery and Mahanuddy, when in flood, the surface used to present to the eye the general appearance of being convex, plane, or concave, according as the river was rising, stationary, or falling, "so plain as to be unmistakable to all who were eye-witnesses of it." All this evidence together is very little; the last is, indeed, only a note of the observer's mental impressions. Now the mind is singularly liable to be deceived in impressions of slight convexity of large areas; *e.g.*, to an observer on a high mountain or in a balloon the distant plain or sea always appears to rise to the level of his eye, so that the earth's surface seems concave to him.

The question is of such high interest that an attempt was made to test it by taking at the same instant the free water-levels at the centre and at both banks in a stream of 171 feet surface breadth, and depth exceeding 10 feet at the centre, and under $\frac{1}{10}$ foot and $\frac{3}{10}$ foot at the two banks, with a surface velocity of about 4½ feet and ½ foot per second at the centre and the banks respectively. The oscillations, amounting to 0·07 foot at the centre, rendered the experiment an extremely difficult one. The centre was found to oscillate above and below the water-level at either edge as much as $\frac{1}{10}$ foot; but the means of twelve trials on one day and twenty-four trials on another, both in calm air and with water gently rising, both gave the very trifling central depression of less than 0·01 foot, so that the fair conclusion seems to be that the water-surface is probably[4] level across on the average.

IX. Sub-surface Velocity.

For all systematic sub-surface velocity-measurement the double-float was exclusively used. Such strong objections have been urged

---

[1] "Recherches Expérimentales sur l'écoulement de l'eau dans les canaux découverts." Pl. xix.-xxvi.

[2] "Mémoires." 2ᵉ série, Tome xvi., p. 29.

[3] "The Royal Engineers' Journal," March 1st, 1882, p. 65.

[4] In a recent review of this experiment, it is said that the true theory shows that this must be level across in permanent motion. Mr. A. Flamant. "Annales des Ponts et Chaussées," Tome iv., 1882, p. 56.

to the double-float that a very full discussion is given in the text, and the adverse opinions[1] are freely quoted. It must suffice to say here that the Connecticut experimenters, having tried both double-floats and current-meters together, decided[2] that the former were "most reliable." The effect of the several inherent faults upon the deduced velocities is also discussed at length. It must suffice to state the principal one, viz., that, in consequence of the current-action on the connector joining the surface- to the sub-float, the efficiency of a given double-float decreases with the depth of the sub-float, so that there is a limit of depth at which it ceases to be useful. The resultant effect of all the faults is, that the sub-float moves at a depth higher than that indicated by the length of connector, so that the velocity-measurement is attributed to a depth greater than the real depth, and is further affected by current-action on the surface-float and connector.

The double-floats used in the systematic work were of two patterns. One had a spherical wood sub-float of 3 inches diameter, loaded with lead, connected by a brass wire 0·012 inch thick to a 3 inches by 3 inches by $\frac{1}{4}$ inch pine disk as surface-float. The other had a spherical copper shell of $1\frac{5}{8}$ inch diameter as the sub-float, loaded with lead, connected by a silk thread $\frac{1}{120}$ inch thick to a 1 inch by 1 inch by $\frac{1}{8}$ inch cork disk as the surface-float. Calling the areas of the sub-float exposed to direct and lateral current-action 100 each, the areas exposed by the surface-floats and connectors at the greatest immersion, 10 feet, were as follows:—

	Surface-float.	Connector (10 feet long).
1st pattern . .	11 direct, 30 lateral	20 direct, 10 lateral.
2nd „ . .	10 „ 14 „	48 „ 24 „

The tension of the connector of the first pattern was ample; that of the second pattern was only 30 grains. It is clear that at the greater depths (say over 6 feet) the connector of the second pattern had too much influence, and that it was not well designed for use in deep water. Mr. Robert Gordon, M. Inst. C.E., the experimenter on the Irrawaddi,[3] describes these floats, after actual inspection, as "models for analysis on a clear-water regular canal:" on the other hand, Mr. Ellis, the experimenter on the Connecticut, reports that they "were not what would be considered of the best form by such American engineers as have had most experience."[4]

---

[1] "Roorkee Hydraulic Experiments," chap. ix., articles 6 to 9.

[2] "Report of the Surveys and Examinations of the Connecticut River," pp. 305, 306.

[3] In a letter to the Author, (August 9, 1878).

[4] In a review on the work, published in "Engineering News." New York, Nov. 26, 1881, p. 478.

X. VERTICAL VELOCITY-CURVES.

There were in all five hundred and sixty-five complete sets of velocity-measurements with surface- and double-floats, at the surface and at every foot of depth below, upon five hundred and sixty-five verticals at three sites. These have been combined into forty-six series (only two containing less than four sets), upon forty-six verticals, viz., twenty-eight central, and eighteen variously situate non-central, one of which was only 7 inches from the edge.

The range of conditions and results was as shown below:—

Work.			Conditions.			Results.
Vertical.	Series.	Sets.	Sites.	Depth.	Surface-breadth.	Mean Velocity.
				Feet. Feet.	Feet. Feet.	Feet per sec.
Central . . .	28	344	3	11·0—3·9	170—82	6·58—2·54
Non-central, at 12½ feet to ⅝ foot from edge	18	221	2	9·6—2·5	169—82	4·27—2·20

The whole is believed to be the most important collection yet published. The mode of combination into series has already been explained; the mean results of series are alone used in discussion. From these means forty-six vertical velocity-curves have been drawn and published. The following are their salient properties:—

"The curves are generally convex down-stream (except near an irregular bank), and are all very flat; their flatness decreases from the centre towards the banks."

"The maximum velocity line is usually below the surface, and sinks in a rectangular channel from the centre outwards to about mid-depth at the banks."

"The mid-depth velocity is usually greater than the mean (on the same vertical), and the bed-velocity is usually the least."

These properties agree closely with those of the curves illustrating Messrs. Darcy and Bazin's small-scale experiments.

The effect of the use of the double-float upon the curves is fully considered in the "Roorkee Hydraulic Experiments," and is shown to be to give them undue flatness, especially near the bed, where the curve is worst determined.

The mid-depth and bed-velocities were computed by simple interpolation for every set, and the means taken out for every series. From the five hundred and sixty-five cases, it is at once seen that the mid-depth velocity is by no means constant, as had been supposed[1] by the Mississippi experimenters, but varies nearly as much as any other velocity. The mid-depth velocity-measurements above have the disadvantage of having been made at various times, sometimes at long intervals. To test this point better, therefore, two special experiments were made; the same mid-depth velocity was measured forty-eight times running with the double-float, and twelve times running with a current-meter, the repetitions being as quick as possible; the ranges of the results were 16·9 and 12·6 per cent. of the mean values, thus fully confirming the above. The statement in the report on the Mississippi is shown in the "Roorkee Hydraulic Experiments" to be based, not upon actual mid-depth velocity-measurement, but upon an argument involving at least two doubtful assumptions.

XI. VERTICAL-CURVE FIGURE.

The figure of the vertical velocity-curve is a question of such high theoretic interest that great pains were taken to investigate it. It is a very delicate inquiry, inasmuch as the available velocity-ordinates are not good data for the purpose; for the figure of the curve depends on its curvature, and therefore only on the "second differences" of the velocities, which are always very minute compared with the velocities themselves, so that a trifling error in the latter involves enormous distortion of the curve. The curves indeed are so flat that probably almost any geometric curve could be fitted pretty close to them. In such uncertainty, the only way of dealing fairly with the case, and especially of obtaining fair values of the parameters involved, appears to be to use the method of least squares. This method was adopted for computing the most probable parabolas to fit the forty-six observation-curves, and also the probable errors; an allowance was made for the decrease of efficiency of the double-float with deep immersion, by varying the "weights" of the velocity-data with distance from the bed, the deeper having the lesser weight. This was a work of great labour, occupying the Author and two skilled computers upwards of a month; much of the labour consists in certain preliminary steps, which can be done once for all: the results of these steps have been recorded in a form which can be used by any good computer.

---

[1] "Report upon the Physics and Hydraulics of the Mississippi River," pp. 294, 295.

The general result is that, except near an irregular bank, the vertical-curve is approximately a common parabola with horizontal axis; but the "probable error" of the computed parameter is often very large.

This last result is of great scientific importance, for it shows that, though a parabola may be formed to fit the observations well enough by other and simpler methods, such as by "trial and error," no confidence can be placed in values of the parameter not formed by the method of least squares, as their probable error is enormous. Similarly formulas for the parameter depending on such values are probably wrong in form, although they may suit well enough for finding a curve to fit the observations, as great changes in the parameter will not unduly displace the curve. A full discussion is given in the text of the parameter-formulas proposed in the Mississippi[1] and in Bazin's[2] experiments; they are shown to be derived from parabolas formed by a process of trial and error, and to fail when applied to new data. After many attempts to construct a new formula, the conclusion is drawn that the data are too uncertain to admit of it.

XII. DEPRESSION OF MAXIMUM VELOCITY.

A lengthened discussion is given as to the cause of the depression of the maximum velocity-line—a point of great interest. It is shown that neither the actual nor the proportionate depression thereof depend much on the depth of water, surface-slope, velocity, or state of wind. This last result is opposed to that of the Mississippi report.[3]

The primary effect of wind appears to be the production of wave-motion, and it causes translation of the water only when long continued; were it not so, every wind would produce a current on a lake or at sea. It seems probable, then, that the short duration of marked up-stream or down-stream wind on canals and rivers prevents it from prominently affecting the sub-surface water, and with it the depth of the quickest stream-line. All modern experiment shows that forward velocity decreases with approach to a

---

[1] "Report upon the Physics and Hydraulics of the Mississippi River," pp. 248–253, 297.

[2] "Recherches expérimentales sur l'écoulement de l'eau dans les canaux découverts," p. 228; and Annales des Ponts et Chaussées, 5e série, Tome x., 1875, p. 313.

[3] "Report upon the Physics and Hydraulics of the Mississippi River," pp. 286, 287.

resisting margin. It appears to the Author that the air itself must be looked on as an efficient upper resisting margin in all open channels; and if air be resistant at all, then it is an ever-present cause of retardation of forward surface-flow. If it be so even in a small degree, the maximum velocity line must necessarily be everywhere depressed, and in a rectangular channel this depression would increase towards, and be greatest (but above mid-depth) at the banks, because the resistance of the wet border, namely, sides and tops, would increase towards the banks. These conclusions agree with the results shown in both Bazin's experiments[1] and in the Roorkee Hydraulic Experiments.[2]

XIII. Discharge past a Vertical.

From the five hundred and sixty-five sets of velocity-measurements at each foot of depth, the five hundred and sixty-five superficial discharges past the forty-six verticals were computed down to the level of the lowest velocity-measurement, by the best approximation-formulas available, including the trapezoidal, Simson's, cubic, and Weddle's, the velocity-ordinates being obviously equidistant; a correction was applied for the lowest space just above the bed. These are considered to be the best values obtainable from the data.

XIV. Mean Velocity past a Vertical.

This quantity[3] is of great practical importance as a step towards computing cubic discharge; its rapid measurement was pointed out in the Mississippi[4] report as the most useful object of present research. Great attention was accordingly given to this. The mean velocities past the forty-six verticals were computed as the quotient of the superficial discharges by the depths for each of the five hundred and sixty-five sets separately. These are considered to be the best values obtainable from the data; and, seeing the number of data from which each is derived, namely, three measurements at each foot of depth, each result may be looked on as a fair average. These are accepted as the fundamental values for testing other proposed approximations. It is important then to show the error thereof due to the use of the double-float. The

---

[1] Atlas, plates xviii.–xxi.

[2] Plates xiii., xvi., xvii., xxx A.

[3] Some good short term is much wanted for this: Mr. L. D'A. Jackson has recently suggested "mean verticalic velocity," which seems very suitable.

[4] "Report upon the Physics and Hydraulics of the Mississippi River," p. 292.

discussion leads to the following simple rule, very easy of application:—

"The double-float mean velocity past a vertical exceeds or falls short of the true value, according as it is less or greater than the surface-velocity."

From comparing the above values and also approximations found by use of loaded rods, described below, arranged in daily groups, it was found that the mean velocity past a vertical is subject to some temporary variation, less than that of the individual velocities.

For rapid approximation, admitting that the average vertical-curve is nearly a common parabola, the properties of the parabola should aid in finding the mean velocity-ordinate by measurement at only a few depths, and therefore far more rapidly than by the tedious process above. It is at once seen that three, and perhaps fewer, ordinates will suffice. This is fully investigated, and several new formulas are given, involving only three velocities, and a more useful new set involving only two velocities. These last are—

$$U = \tfrac{1}{4}\left(v_0 + 3\,v_{\frac{2}{3}\mathrm{H}}\right) = \tfrac{1}{7}\left(3\,v_{\frac{1}{6}\mathrm{H}} + 4\,v_{\frac{3}{4}\mathrm{H}}\right) = \tfrac{1}{7}\left(4\,v_{\frac{1}{4}\mathrm{H}} + 3\,v_{\frac{5}{6}\mathrm{H}}\right),$$

wherein U is the mean velocity sought, v the velocity at the depth shown by the subscript, and H the actual depth on the vertical. Another, since discovered by the Author, by pursuing the same investigation, is:—

$$U = \tfrac{1}{2}\left(v_{\cdot 211\,\mathrm{H}} + v_{\cdot 789\,\mathrm{H}}\right).$$

The investigation shows that these are the simplest formulas obtainable. The first is one of the best[1] for practical use, inasmuch as the velocities are measured at the highest levels possible in such a formula, and are therefore the most easily measured.

It is important to inquire whether a single velocity would suffice. This would be possible if the velocity at any definite depth were equal to the mean; but this depth is found to depend upon the position of the maximum velocity-line, and is therefore variable. From the flatness of the curve an approximation is, however, possible. It is shown that the velocity at $\frac{5}{8}$-depth, or at $\frac{7}{10}$-depth is a fair approximation according as the maximum velocity-line is above or below $\frac{1}{4}$-depth; the former case usually obtains, the latter only near a vertical bank. In the Mississippi report[2] it is proposed to use the mid-depth velocity for this approximation. Much atten-

---

[1] It is considered by Mr. Ellis, "Engineering News," of Nov. 26, 1881, p. 479, to be "the best of all approximation formulas yet suggested."

[2] "Report upon the Physics and Hydraulics of the Mississippi River," pp. 293-296.

tion was therefore given to this. It is easily shown that, the average curves being very flat and convex down-stream, the mid-depth velocity must, whatever be the curve, exceed the mean velocity by a small quantity; in the forty-six average curves of this work there is only one marked exception to this. In the Mississippi Report,[1] the ratio of the mean to the mid-depth velocity is said to be a "sensibly constant quantity for practical purposes." Were this true it would be an important result, but the experimental evidence now available shows that the ratio varies from 1·082 to 0·918, or about 16 per cent., a quantity not fairly negligible.

The first formulas above have the disadvantage of requiring measurements at two points by two operations. In the last and newest formula, however, only an arithmetic mean is needed. This permits of the two velocities being measured together by a suitable instrument, a great practical advantage; this being a new and important result requires fuller explanation here. It has been shown by the Author[2] that, if a double-float be made up of two sub-floats of equal size, similar shape, and similar surface physically, which move at different depths, say λ H, μ H, in strata whose velocities are $v_{\lambda H}, v_{\mu H}$, then the velocity, say u, of such an instrument will be—on the usual theory of current-pressure and friction on immersed solids—simply the arithmetic mean of those velocities. Hence, if the sub-floats be sunk to depths of 0·211 H and 0·789 H, the velocity (u) of the instrument will by the last formula be actually the mean velocity (U) required, which is thus obtained at one operation with a single instrument; this effects a great saving of time and labour. But another great advantage accrues; the upper sub-float may be made very buoyant, and the lower heavy, so as to throw considerable tension on the connector between them, thus getting rid in great part of one of the worst faults, namely want of stability, of the ordinary double-float; so that the results should be improved in accuracy. The Author would recommend that the sub-floats should be thin copper shells not less than 2 inches in diameter, connected by a fine silk thread; the surface-float a small slice of cork joined by silk thread to the upper part of the sub-float. For shortness this might be called the "twin balls." This instrument appears to the Author the best yet proposed for depths exceeding those suitable for loaded rods.

All approximations by measurement as above at only one or

---

[1] "Report upon the Physics and Hydraulics of the Mississippi River," pp. 294–296.

[2] "Hydraulic Experiments at Roorkee," 1874–75, art. 49.

two selected points require, of course, frequent repetition to bring out average values, as, in consequence of the unsteady motion, it is only these averages that can be expected to approximate to the required mean velocity of the average curve.

Lastly, an attempt was made to find an expression for this mean velocity in terms of the depth, surface-gradient, &c.; it was found to depend much more on the latter than on the depth, but the actual relation could not be traced. It was also found that the direct measurement of almost any velocity was far more likely to give an approximation to this mean velocity than any expression yet known not involving velocities.

XV. RODS.

The use of a loaded rod or float-pole has often been recommended for rapid approximation to the mean velocity past a vertical. It obviously gives some sort of mean of the velocities at all parts of its immersed length, but the degree of approximation has not been hitherto sufficiently investigated.[1] This point was taken up pretty thoroughly at Roorkee.

Out of the five hundred and sixty-five sets above-mentioned, thirty-six sets were specially done with a complete double equipment of both instruments, double-float and rod, of 1 foot, 2 feet, 3 feet, &c., to 7 feet depth of immersion. The two instruments were used together in pairs of like lengths, so as to secure the same state of water for both; and every velocity was thrice measured as usual. The results were grouped into six series, three for each instrument; lastly, the average rod-velocities of the several lengths were compared with the average mean velocities past the upper 1 foot, 2 feet, 3 feet, &c., to 7 feet of each vertical, computed from the double-float work; there were in all eighteen pairs of comparable results.

Again, along with five hundred and forty-three of the above-described sets of double-float observations, five hundred and forty-three rod-velocities were measured each six times with rods of nearly full immersion. These rod-velocities are printed set by set along with the five hundred and forty-three mean velocities past each complete vertical computed from the double-float work. The great range of the conditions and data in that work, already described, gives high value to these results.

---

[1] In the "Lowell Hydraulic Experiments" (art. 179, 180) eleven experiments on this point are detailed; this number is too few for broad generalisation.

The grand result was that, after making due allowance for the known inaccuracies of the double-float, the rod-velocity is most probably a closer approximation to the mean velocity past its vertical than the value deduced from the double-float. Observe that this result is purely experimental.

Rods of two patterns were used, namely, 1-inch cylindrical wood poles loaded with lead at the foot, and 1-inch tin tubes made of stout sheet tin, loaded at the foot with a short length of rod-iron, and closed at the ends. The latter was found to be by far the best.

The following are the theoretical advantages claimed for the rod, over the double-float, for measurement of mean velocity past a vertical:—It is free from the uncertainty attending the instability and lift of the sub-float; and the result is a closer approximation than that given by the double-float. The practical advantages are, that the result is obtained much more quickly; that the rod is more easily handled, and that it is simpler in construction, less delicate, and cheaper. These advantages are so great that it seems to the Author that it should supersede all other instruments for the purpose in conditions favourable to its use. The necessary favourable conditions are:—a reach of nearly uniform cross-section and average bed-slope for a great length; that the bed and banks should be pretty even lengthways near the site; and that the depth should not exceed 15 feet. It would often be worth while to prepare a site, by dressing the bed to the average bed-slope, and the banks to a uniform side-slope for at least 250 feet length; and the banks would be better if revetted with masonry.

XVI. ROD-MOTION.

The experiments did not show directly whether the rod-velocity was greater or less than the mean velocity past the vertical. This was supplied by a mathematical investigation of the motion, but so complex and so long that it can merely be indicated here. It is based on the assumption that the resultant forward force on any part of the rod varies as the square of the relative forward velocity of that part and the fluid adjacent, that the resultant force on the whole rod is zero when in relative equilibrium; and that the figure of the vertical velocity-curve is a parabola. Upon this it has been found possible[1] to solve the equations of motion, with the practical result that the rod-velocity is always somewhat less

---

[1] Now done for the first time.

than the mean velocity past its own immersed length, and that, finally, to measure mean velocity past a vertical of depth H, the rod should be immersed only about 0·94 H. As practical necessity also obviously involves the use of a rod immersed decidedly less than the full depth, this result is of great importance, as it removes one of the chief objections hitherto urged to the use of rods,[1] namely, that in consequence of not reaching into the slack water near the bed, they move quicker than the mean velocity.

XVII. TRANSVERSE VELOCITY-CURVES.

There were in all seven hundred and fourteen complete sets of velocity-measurements at from eleven to twenty-one selected points on seven hundred and fourteen transversals. The transversals were the surface, mid-depth, bed, and a quasi-mean, the last being so-called because mean velocities past many verticals scattered across the channel were measured. The instruments used were surface-floats for the surface, double-floats for the mid-depth and bed, and rods for the mean transversal respectively. The whole have been grouped into one hundred and fourteen series on as many transversals at thirteen sites.

Observations.			Conditions.				Results.	
Transversal.	Series.	Sets.	Sites.	Central Depths.	Surface-Breadths.	Surface-Slope per million.	Mean Velocity.	Discharge.
				Feet. Feet.	Ft. Ft.			
Surface . .	10	109	4	10·3–7·5	169–82	233–178	4·61–3·60	708–306
Mid-depth .	2	17	1	10·1–9·1	84–82	210	4·54–4·10	378–348
Bed . . .	2	7	1	10·0–8·7	84–82	..	4·24–3·42	361–291
Mean . .	100	581	11	11·2–0·7	193–13	480–24	4·87–0·69	7,364–25

The rod-velocity work was considered by far the most important, as from it the cubic discharge, the final object of the whole work, was to be computed; and it will be seen that five hundred and eighty-one sets of observations were of this sort. The range of the external conditions, and therefore also of the results, was very great. The whole is believed to be the most important collection yet published. The mode of combination into series

---

[1] "Lowell Hydraulic Experiments," art. 179; "Report upon the Physics and Hydraulics of the Mississippi River," p. 309.

has already been explained. Owing to canal exigencies low water occurred so seldom, and lasted so short a time, that it was impossible to repeat such work often in the same state of water; thus in the rod-work there are twenty-nine series containing less than five sets each, besides twenty-two of only a single set; so that the low-water work at each site is of less weight than the rest. The mean results of series are alone used in discussion in general.

The spacing of the float-courses was fixed so as best to exhibit the figure of the transverse curves; and so as to be convenient for discharge-computation. To meet the former the float-courses were spaced widest where the curve was known to be flattest, viz. throughout the central portion, and closest where the curvature was known to be sharpest, viz. near the banks; to meet the latter the spacing was made equidistant throughout the central, intermediate, and side spaces.

From these series one hundred and fourteen curves have been drawn and published; these are the transverse velocity-curves. The following are their more salient properties:—

"The curves are generally convex down-stream over a level or concave bed, are nearly symmetric in a symmetric cross-section, and are all very flat."

"The velocity is generally greatest near the centre (or deepest channel), decreases very slowly at first towards both banks, more rapidly with approach to the banks or with shallowing of the depth, and very rapidly close to the banks."

"The figure of the curve is determined by the figure of the bed, increased depth producing increased velocity, and *vice versâ*, so that a convexity in the bed produces a concavity in the curve, and *vice versâ*; also these effects are more marked in shallow than in deep water."

The decrease of forward velocity is so rapid close to the banks, as to make it clear that the forward velocity at the edges must be very small, possibly zero. It does not admit of direct measurement. The employment of floats close to the edge is of course impossible when the edge is irregular; but even with artificial straight banks their use is very difficult in consequence of a prevailing transverse surface-current from the edges, which is so marked that in a float-course only 7½ inches from a straight vertical bank, occasionally one hundred surface-floats were run before three were obtained in fair course over a 12½-feet run. This transverse surface-flow is supposed by some to be caused by the reduction of pressure at the centre consequent on the higher central velocity.[1] To keep up the water-level at the edge, this surface-flow from the edge clearly involves a sub-surface-flow towards the edges; the experiments showed this indirectly, in that the deeply-immersed double-floats and rods moved without any general bias.

XVIII. TRANSVERSE CURVE FIGURE.

The geometric figure of these curves is a matter of high scientific interest. The unsuitability of the data (just as in Section XI.), and their extreme flatness makes it a delicate matter; hence the velocities near the banks, where alone there is any marked curvature, are the most important for this purpose; in fact, omitting these, almost any geometric curve might be made to fit. This is well shown in the trials of the late Mr. Darcy and Canon Moseley, who proposed a semicubical parabola[2] and an exponential curve[3] respectively, from certain theoretical investigations; and, though using the same experimental data, Darcy's Results in Pipes, for their verification, were each satisfied with their own results; Darcy's curve, however, is convex, whilst Moseley's is concave down-stream; but the experimental test is very poor, as Darcy's velocity-measurements were solely in the middle two-thirds of the stream, thus omitting the critical portions near the edges.

One interesting general result is deduced in the discussion, that "Curves of like kind with same water-level at same site, but with different general velocities, are nearly parallel projections of one another." This is deduced from a consideration of five pairs of curves of the same kind, the two curves of each pair being obtained at nearly the same water-level at each site, and each containing fifteen or sixteen measured ordinates for comparison; the cases, five in number, are few for so broad a generalisation, but they are each good in that the general velocities in the two curves of each pair are very different, their ratios being as 3·4, 4·7, 1·7, 2·3, 1·9 to 1 respectively.

Two other important conclusions are drawn. No single curve will suffice, as the flatness of the curve varies with the water-level in such a way as to show that the exponents of the abscissas should

---

[1] "On the Steady Flow of a Liquid." Philosophical Magazine, vol. xlii., p. 353; vol. xliv., p. 44.

[2] "Recherches expérimentales relatives au mouvement de l'eau dans les tuyaux," p. 128.

[3] Philosophical Magazine, vol. xlii., p. 192.

probably vary with the water-level. And no single species of curve will suffice, inasmuch as it seems that the figure of the curve is, for given states of water, determined by the figure of the bed. It follows that it is almost hopeless to seek the figure of the curve from mere experiment, without some help from a rational theory.

XIX. Areas and Discharges.

From the average soundings, and from the above-mentioned sets of velocities past a transversal, the whole of the cross-sectional areas required and the corresponding discharges, thus including five hundred and eighty-one cubic discharges, were computed by the most accurate formulas available (viz. the trapezoidal, Simson's, cubic, and Weddle's) for a curved area or volume divided by equidistant ordinates or planes. For the cubic discharge, the most important of all, which is represented by the volume of the velocity-surface, the preliminary step was to multiply[1] every rod-velocity (u) by the average depth (H) in its float-course, the products (Hu) being the superficial discharges past each vertical, *i.e.* the areas of the equidistant plane sections of the velocity-surface; these areas being known, the above formulas were at once applicable. It might be supposed that the use of these formulas is very troublesome; they are, on the contrary, very simple, and their use was readily acquired by the ordinary overseers of the Indian P. W. D. It is shown in the text that, on account of the usual convexity of the curves with concavity of the bed, all simpler formulas err in defect in the long run, and in the case of the cubic discharge the error is not necessarily very small. A curious source of error occurs in that in these formulas different ordinates carry different coefficients, and have therefore different weights in the result; thus in Simson's formula, the middle ordinate being multiplied by 4, an error in it is four times as important as a similar error in the end-ordinates; to avoid this it would seem necessary to have all the ordinates of equal weight, by making the number of repetitions of each measurement proportional to its weight in the formula; this would be so troublesome that it would only be done to secure a high degree of accuracy. The present results are considered to be the most accurate obtainable from the data.

The surface, mid-depth, and bed-discharges being of little

---

[1] The direct multiplication would have been very heavy work, but the use of Crelle's Tables, which show products up to 999 × 999, made it easy.

interest will not be further alluded to here. The cubic discharge is so important that it seems well to recapitulate the process of measurement used. This contains three distinct steps: 1. Obtaining the average depths by sounding along a number of float-courses. 2. Rod-velocity measurements in each float-course. 3. Computation. The time required depends on the number of float-courses, and on the number of repetitions of the measurement of each depth and of each velocity. Closer approximation to average results is obtained by increase of these numbers. At Roorkee the details were: 1. Sounding along about sixteen float-courses in eight cross-sections; time, three to four hours. 2. Rod-velocity measurements in fifteen to twenty-one float-courses, each thrice repeated; time, two to four hours. 3. Computation, about two hours. The chief advantages of this process are that it is direct and purely experimental, and therefore independent of any as yet uncertain theory; that the data are taken from many parts of a site, so that the result is certainly some sort of average; and that the whole velocity-work is done within a short time, *i.e.* in as nearly a constant state of water as is practicable. This last point is of extreme importance.

From the mode of computation it is evident that breadth, depth, and velocity all enter as factors into the result. In wide streams the breadth varies but little, so that the cubic discharge varies chiefly with the other two factors. The experiments show clearly that the discharge increases and decreases with the rise and fall of the water-level, but depends in a far greater degree on the velocities. This shows that a discharge-table for a given state of water must be a table of at least double entry, showing the discharge as dependent on both gauge-reading and velocity, or some equivalent data, *e.g.* gauge-reading and surface-slope, or gauge-readings throughout the reach; this result, which seems self-evident, is of great practical interest. The official tables in use on the Ganges Canal have hitherto indicated a definite discharge for a given depth, but the power of control on this canal is so great that the gauge alone is no indication of the discharge: witness the following results:—

	Solání R. Aqueduct.			Solání Embankment.			
Gauge-reading . .	4·6	4·0	3·6	3·6	3·6	3·6	2·9
Cubic discharge . .	482	1,623	212	1,124	643	483	1,142

On comparing the new results with the official canal tables, the new results were found to be unexpectedly higher than the official at all the higher depths. This was at first supposed to be due to an inherent defect in the use of rods which had hitherto been believed to register unduly high velocities, in consequence of not reaching into the slack water just over the bed; but if this were so, the excess in question would be relatively largest at low water, because a small lift of the foot of the rod above the bed is of greater relative importance in shallow than in deep water, whereas at low water the new results were found to be less than the official. The supposed defect of the rod has, however, been shown above not to exist. The mode of preparation of the official tables was examined, and is demonstrated to be such as to lead naturally to under-estimation at high water, and to over-estimation at low water.

One chief aim was to compare the experimental discharge-measurements with those given by various discharge-formulas; most of these formulas give the mean velocity; the comparison of mean velocities is in these cases most convenient, and will be discussed later. A somewhat complex discharge-formula was proposed by the late Canon Moseley,[1] and was shown by him to agree pretty well with Bazin's small scale experiments; it is interesting as having been deduced from a rational, but probably incorrect, theory of fluid motion. On comparison with the present large scale results, it was found to give results of only from $\frac{1}{4}$ to $\frac{1}{14}$ of the observed, so that it is clearly useless.[2]

XX. MEAN VELOCITY.

From the discharge-measurements above-mentioned, the corresponding mean velocities were computed separately for each set by dividing each cubic discharge by the area, thus giving five hundred and eighty-one mean sectional velocities. These are considered to be the best values obtainable from the data, and seeing the number of data from which each is derived, namely three measurements at from eleven to twenty-one points on each transversal, may be looked on as fair averages. The mean surface, mid-depth, and bed-velocities, being of little interest, will not be further alluded to here; the very important mean sectional velocity is, for shortness, called "mean velocity" below.

It seems clear that the cubic discharge is constant from instant to instant, so that the mean velocity must be so likewise, *i.e.* both are technically steady. The measurements of both these quantities,

---

[1] "On the Steady Flow of a Liquid," Philosophical Magazine, vol. xliv., p. 46.

[2] The Author has not seen this formula challenged before.

however, indicate marked and seemingly capricious variations in apparently the same state of water; no doubt due to their being simply an average from a number of incessantly varying data. The cubic discharge and area both vary with change of water-level, but the effect of this disappears in part from their quotient, so that the mean velocity must be less variable than the discharge.

The mean velocity V is a quantity of such great practical use for immediate computation of the all-important cubic discharge, as the product $D = A\,V$, that immense labour has been given at various times in the endeavour to find some rapid approximations to it. These have taken two principal forms, one involving only velocity data, and the other only surface-slope and cross-section data. The most important modern trials of the latter, by Mr. Bazin and Kutter, are based on the old Chézy formula, involving $\sqrt{R\,S}$.

On this work data were collected on an extensive scale for trial approximations in three ways. In fact, all the experiments were directed to this. Measurements were made of three quantities, viz.:

Central[1] mean velocity U_o, central surface velocity v_o, surface-slope S,

the approximations being intended to be made by using certain "reduction-coefficients" (α, β, C), thus:—

$$V = \alpha\,U_o;\ V = \beta\,v_o;\ V = C\,w,$$

where for shortness[2] $w = 100\,\sqrt{R\,S}$. The experimental values of the coefficients are computed by inversion of these formulas.

The use of the central mean velocity U_o in this way was an after-thought; it had been measured as a part of the regular work of each of the five hundred and eighty-one above-mentioned sets of rod-work, thus giving five hundred and eighty-one values of both U_o, V. Those of U_o are poor averages,[3] having been only thrice repeated; the reduction coefficient $\alpha = V \div U_o$ has therefore been taken out only for the one hundred mean values of U_o, V in the one hundred series into which the rod-work was grouped (*supra*). The central surface velocity v_o, and surface-slope S, were measured

---

[1] A short term denoting "mean velocity past the central vertical."

[2] This important quantity w is conveniently treated of as a quasi-velocity.

[3] Had this work been foreseen, each velocity would have been forty-eight times measured.

along with many of the five hundred and eighty-one sets of rod-work just mentioned. They are both so much affected by wind that they were taken, as a rule, only when wind and weather were favourable; thus only three hundred and thirteen values of v_o, and three hundred and sixty-three of S, were obtained. These were grouped, along with the corresponding values of V, into special series, namely, seventy-six of v_o, V, and eighty-three of S, w, V; the reduction-coefficients $\beta = V \div v_o$, $C = V \div w$, were taken out for each set separately, as well as for the means of series. The central surface-velocity was always measured forty-eight times, so is a good average; the surface-slope was measured once only with each set on one bank in two hundred and twelve sets, and on both banks in one hundred and fifty-one sets. The range of conditions and results was very great, nearly the same as that of the rod-work, *q.v.* The whole form, it is believed, one of the most important collections of such data yet published. The "weights" of V, U_o, v_o, w, will be seen to be very different, and the series containing them do not correspond; this could not be helped. As a general rule, only the serial means of the four velocities (V, U_o, v_o, w), and of the three coefficients (α, β, C) have been used in discussion. From these data diagrams have been published to show the relations of these quantities to each other, and to various hydraulic elements. From the tables and diagrams it is evident that:—

The four velocities, V, U_o, v_o, w, increase and decrease, as a rule, together, and also increase and decrease in general jointly with the increase and decrease of hydraulic mean depth and surface-slope.

"The connection between V, U_o, v_o, seems much closer than between V, w."

"Up- or down-stream wind markedly decreases or increases surface-velocity; its effect on mean velocity seems quite trifling."

Next, as to the coefficients α, β, C, it appears that—

"The ratio C increases with decrease of R, but depends largely on some other unknown elements. The variation of β is irregular (probably partly from wind effect). The ratio C increases with increase of R, and depends also greatly on S, and also on the nature of channel."

After discussing various known formulas for mean velocity, the only ones that appeared worth extended trial were Bazin's[1] for-

---

[1] "Recherches expérimentales sur l'écoulement de l'eau dans les canaux découverts," p. 125 *et seq.*, p. 157 *et seq.*

mulas for the coefficients β, C, and Kutter's[1] for the coefficient C. Accordingly the values of these coefficients, from the published[2] Tables, have been printed alongside the experimental mean serial values, seventy-six of β and eighty-three of C. As to Bazin's two coefficients (β, C), the discussion shows that neither is reliable, and that the use of the former with[3] surface-velocity leads to under-estimation of mean velocity, and that the latter is defective in not containing S. As to Kutter's coefficient C, the discrepancies between the eighty-three experimental and computed values were:

Thirteen over 10 per cent., five over 7½ per cent., fifteen over 5 per cent., seventeen over 3 per cent., thirty-three under 3 per cent.

Now in all the discrepancies over 10 per cent., it was found that the state of water was unfavourable for the slope-measurement. Taking this into account, along with the varied evidence in Kutter's work, it seems fair to accept Kutter's coefficient as of pretty general applicability; also that when the surface-slope measurement is good, it will give results seldom exceeding 7½ per cent. error, provided that the rugosity-coefficient of the formula be known[4] for the site. For practical application extreme care would be necessary about the slope-measurement, and the rugosity-coefficient could only be determined, according to present knowledge, by special preliminary experiments at each site.

The formula derived from the Mississippi experiments[5] was also tried in nineteen test cases; the agreement with experiment was extremely poor. This has already been shown in Kutter's work.

The experimental results (V, S) of this work have been recently applied[6] by Dr. Hagen to test his proposed new general formula $V = C\,R^m\,S^n$; from a selection of forty-three of the series most complete in the data (V, S) he deduces, by the method of least squares, $V = 65\ R^{\frac{1}{3}}\ S^{\frac{1}{4}}$; but the "probable errors" computed therewith appear enormous. Moreover, he had in the same way

---

[1] "The New Formula for Mean Velocity of Discharge of Rivers and Canals." By W. R. Kutter. Art. 7.

[2] "Professional Papers on Indian Engineering," vol. v., 1868, pp. 283, 294, 295. "Canal and Culvert Tables." By Lowis D'A. Jackson.

[3] The formula is based on maximum velocity, but is commonly used with surface-velocity.

[4] In the test-application described those values were chosen out of Jackson's Tables which agreed best with the experiments.

[5] "Report upon the Physics and Hydraulics of the Mississippi River," p. 312.

[6] "Zeitschrift für Bauwesen," 1881, p. 408.

deduced from the Mississippi experiments the different expression $V = 6\ R^{\frac{3}{4}}\ S^{\frac{1}{2}}$. From this he concludes that probably the surface-slope measurements are too inaccurate for use in such a formula.

The general result of trial of these formulas, which are all empirical, shows that at present increased approximation can only be obtained by increased complexity;[1] there is no guide as to the form of such improved approximations, whilst the labour[2] of tentative research is excessive. Until some guide is obtained from a rational theory, it seems to the Author hopeless to attempt further improvement.

In the absence of any true formula, that coefficient which is the least variable would probably be the best for practical use, as likely to lead to least error. Now the range of the experimental values of $C = V \div w$ is far larger than that of either a or β. This result is of great importance, as showing that the connection between mean velocity and other velocities is far closer and more intimate than between velocity and surface-slope. The connection is unknown in any case; but the latter is a physical one, requiring the determination of velocity from physical conditions, whereas the former is possibly only a geometrical one, depending on the figure of the velocity-surface. Moreover, the surface-slope is extremely difficult of proper measurement, and its use involves a working from the minute to the large. It seems, then, that at present the direct measurement of velocity, such as the central mean or central surface, is more likely[3] to give a near value of the mean velocity than any formula involving surface-slope, and of the two velocities the central mean is to be preferred, as being little affected by wind; but there is as yet no good formula for reducing these velocities to the mean, so that the "reduction-coefficients" required must at present be determined by special trial for each site.

XXI. Discharge-Verification.

It is very important to have the means of testing the accuracy of any process of discharge-measurement. On the large scale any absolute test is impracticable. The only test that seems practicable is that the results should be consistent with each other. Many of the Roorkee results admit of such a test being applied in several ways.

---

[1] Witness the increase of complexity from Chézy's to Kutter's formula.

[2] Kutter's Discussion, as translated by Lowis D'A. Jackson, covers 95 octavo pages, without the numerical detail.

[3] Compare the results as to mean velocity past a vertical (sec. xiv.).

Test 1.—It is clear that discharge-measurements, and therefore also the deduced mean velocities, made at the same site under nearly similar states of water should be nearly equal. The different sets of rod-velocity work from which they are computed were seldom done at exactly the same water-level. The difference of water-level affects the computed discharges directly, but scarcely affects the deduced mean velocities (Sec. XX.), so that the latter are the more fairly comparable.

Test 1*a*.—A series being a group of data, &c. in nearly[1] the same state of water, the mean velocity results within each series can thus be compared together. Rejecting, out of the total of one hundred series[2] of rod-work, the twenty-two series containing only one set each, there remain seventy-eight series containing five hundred and fifty-nine sets in all, which yield two thousand seven hundred and fourteen pairs of comparable results. There were only eighty-eight cases of discrepancy over 10 per cent. in this large number. On examining the details of these, it was found that the external conditions were in a large number not nearly so closely similar as was expected from their collocation within same series, or as is desirable in such a test.

Test 1*b*.—Out of the whole number of five hundred and eighty-one. sets,[2] two hundred and fifty-six sets had been done in one hundred and six days, viz. in one hundred and six groups of 2, 3, 4, 5, or 6 within the same day, usually in immediate succession, at the same site. These yield one hundred and ninety pairs of comparable mean velocities, which seem favourable for this test, as the probability of constant state of water throughout one day is considerable. The discrepancies were really very small, being over 10 per cent. in seven groups; over 5 per cent. in two groups; over 3 per cent. in seventeen groups; under 3 per cent. in eighty groups; and in every one of the cases over 5 per cent. the cause was traced to the canal being out of train.

Test 1*c*.—On one occasion two complete sets of rod-velocity work were done by timing the whole of the rods through both a 50-feet and a 100-feet run. The discrepancy of the mean velocities deduced was only $\frac{3}{10}$ per cent.; this extreme closeness is probably accidental.

Test 2.—It is clear that discharge-measurements at successive sites, between which there is neither influx nor outflow, in the same stream should be nearly equal, if conducted under nearly similar general external conditions in the portion of the stream

---

[1] Sec. vi.

[2] Secs. xvii., xx.

between the sites. This test can be applied to seventy-five cases, by comparing the discharge at an upper site with that at a single lower site in ten cases, and with the sum of discharges at two lower sites (in two branches of the stream) in sixty-five cases. The sites being of very different natures, widths, and depths, the test is a searching one.

Test 2*a*.—In the first six trials the fieldwork at the upper and lower sites was done on different days, and therefore under somewhat different external conditions. A small correction was applied to reduce the work at each site to a common level at the standard gauge; the highest discrepancy remaining was 7 per cent., and the rest were under 5 per cent.

Test 2*b*.—In thirty-five more trials the fieldwork was done at the same time at the upper and lower sites by three complete field-parties; the discrepancies were, six over 5 per cent. (8·3 the highest); six over 3 per cent.; twenty-three under 3 per cent.

Test 2*c*.—In thirty-four more trials the fieldwork at the two lower sites was done later than at the upper site, time being roughly allowed for the water to pass from the upper to the lower sites; these may be said, in a rough way, to have been carried out in the same body of water; the discrepancies were, two over 5 per cent. (5·5 the highest); three over 3 per cent.; twenty-nine of 3 per cent. and under.

In very many of the cases of the higher discrepancies in each test certain disturbing causes were found to exist, *e.g.* a rising or falling state of water, high wind, &c.; some of the sites were also unfavourable. The evidence of all the tests together as to the consistency of the results is very great both in amount and in range of data. The application of Test 2 was of course expensive, as it involved the use of three complete field-parties for six months; the importance of these tests was well worth the expense.

The conclusion seems fair that "the process of discharge-measurement described yields, under favourable circumstances, results which will probably seldom differ 5 per cent." For such close approximation it seems essential that—1. The sites should be favourable; 2. Fieldwork should be done only when the water is "in train."

XXIII. Current-meters.

Three current-meters, one of Moore's, one of Révy's, and one of Elliot's patterns, were tried for some time, but the experimental difficulties were not got over. A pretty full discussion is given of the disadvantages of current-meters in general and of the means of their improvement. A special lift was contrived for gripping the meter firmly parallel to the current-axis, so as to register only forward velocity, and having a continuous rigid gearing wire. This got rid of at least four bad faults, viz. the uncertainties of orientation and of position of the meter, and of gearing and ungearing it, and the non-measurement of forward velocity which occur otherwise in the ordinary use. A great improvement might be made so as to reduce the mass placed in the water, viz. by separating the recording works from the screw and placing them above water under the observer's eye, with electric connections to cause them to follow the motions of the screw. But at best, there are serious difficulties in their use, some of which seem insurmountable.

XXIV. SILT.

An important course of experiments was made on the distribution of and amount of silt in the channel. A specimen of the water was collected in a brass tube 12 feet long by 2-inches internal diameter, open at both ends. This was thrust vertically from a boat floating freely down stream, until it touched the bed, whereon the lower end was closed by a movable lid worked by a strong spring; by this means a column of water stretching from the surface to the bed of the channel was collected. The silt was separated from the water by decantation and filtration, and was finally weighed on a well-dried filter in a chemical balance. From this the weight of silt in grains per cubic foot of water was computed. This result is called silt-density; it is obviously an average of the vertical of collection. Two styles of experiment were made, one on the distribution of silt across the channel, the other on the relation of the silt-density to the velocity and depth.

Silt-distribution.—Two sets of silt-collections were made at nine points, distributed across two of the large sites, each set being done as quickly as possible. Each resulting silt-density multiplied by the mean velocity past the vertical of collection is obviously the mean silt-velocity past that vertical. Next, the three quantities, namely, the silt-density in, the mean silt-velocity past, and the mean velocity past each vertical, were plotted as ordinates on a common base line, thus giving three transverse curves. These curves were so little alike and so irregular, that it seems evident there is no close connection between the silt and the velocity at different parts of a channel, also that probably the silt-density at any point varies from instant to instant, *i.e.* is, technically, unsteady.

Silt-relations.—Again, seventy-three collections were made on the central vertical at four of the large sites in very varied conditions of depth and velocity at two of them. From these it appears that the silt-density in no way depends on the depth or velocity, at any rate in the Ganges canal. In fact it seems certain that the silt in this canal depends chiefly on the state of the supply-water from the Ganges, which varies from clearness to great turbidity, and on the occasional admission of local drainage which is often very turbid; so that the canal is unsuited for experiments on the relation of silt to velocity, &c. This is a disappointing conclusion, as the labour of the silt-collection and reduction was very great.

XXV. Evaporation.

An attempt was made to measure the evaporation from the canal surface, lasting twenty-five months from 1876–79. The evapometer was a zinc pan 12 inches square and 9 inches deep, above which the four sides splayed outwards so as to form a "free-board" 10 inches high and exposing an opening of 30 inches square at top to the sun and air; this rested in a wooden frame buoyed by zinc air-chambers so as to float in water. The experiment was started by pouring canal water into it to a depth of about 6 inches; after carefully measuring this depth, the pan was set afloat at mid-channel, moored in a place where it was not likely to be disturbed; when so floating, the water-level inside the pan was nearly flush with the surface of the stream. After about a week, it was taken out and the depth of water remaining was carefully measured. The loss, if any, was considered to be the evaporation, diminished by dew.

Out of the twenty-five months mentioned, measurements during two were lost by an accident to the pan, and eight months of "rainy season" were also unavailable; in the remaining fifteen months about half the trials were vitiated through stress of weather; *e.g.* every trial affected by rain had to be rejected until the erection of a rain-gauge at the site enabled an allowance to be made for rain. Thus, finally only forty results were obtained. Along with twenty-eight of these results, the mean temperature, mean humidity, and mean wind at the Roorkee Observatory are recorded; also, in a few cases, the temperature of the canal water.

The most remarkable feature of the results is their extreme smallness, amounting to only about $\frac{1}{10}$ inch per day on the average near Roorkee; whereas $\frac{1}{2}$ inch per day is said to be a common rate in India for evaporation on land. This led at first to the suspicion of the introduction of water from without; but after considering the possible sources of this, namely, leakage, spray, rain, dew, wilful tampering, it still seems that the results may be accepted as substantially correct. The real cause of the small evaporation appears to be the unusual coldness of the canal water, *e.g.* on May 22, 1877, at 2.30 P.M., the temperature of the air was 165° in the sun, and 105° in the shade, whilst that of the water was only 66° inside the pan and 65° in the canal; also the highest recorded temperature of the canal water was only 75½°. The canal, in fact, takes its supply from the Ganges, a snow-fed river, at its exit from the hills.

It was, indeed, found that the canal-evaporation increases with distance from the head, *i.e.* from the Ganges. Thus out of the forty results, twenty-eight were taken near Roorkee, and twelve near Kamhera, at distances of 18 and 52½ miles from the head-works; the evaporation at the latter was much the larger, comparing, of course, similar seasons, being about 0·15 inch against 0·10 inch on an average. This is no doubt due to the gradual heating of the water under the hot sun with increased distance from the head.

Taking the Roorkee estimate of $\frac{1}{10}$ inch per day, the total evaporation from the whole surface of the canal and its branches, about 487 million square feet, amounts to about 47 cubic feet per second, which is about $\frac{1}{150}$ part of the full supply of the canal, or in other words ten minutes' full supply daily.

Little connection could be traced between the evaporation and the meteorological elements; the temperature of the water, which depends chiefly on the amount of snow-water in the Ganges, being probably the governing element.

Discussion.

Major Cunningham.

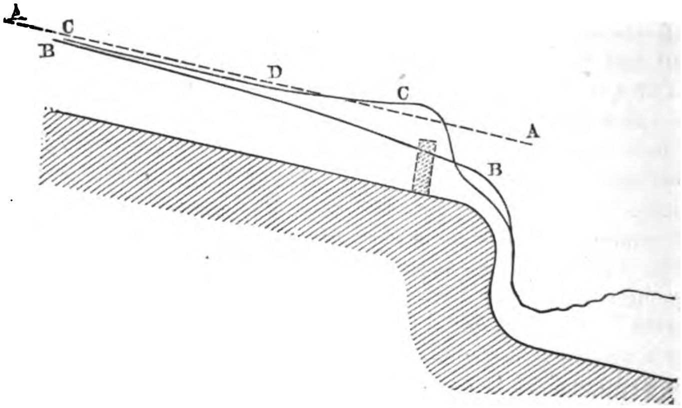
Major **Allan Cunningham** desired to add that the experiments, of which an account had been read in abstract, were very extensive, the complete report of them occupying three large volúmes. It had been difficult to prepare a condensed report of the work which should give a fair account of it, and at the same time not be tedious to the meeting. The subject of the flow of water was an important one, both scientifically and financially. It had been stated that the crops which were saved in a single season of drought by irrigation from the Ganges canal, on which the experiments were made, yielded a revenue which covered the entire cost of the construction of the canal—a revenue which otherwise would have been entirely lost. The subject was a dry one; but Figs. 6 and 7 (page 64) showed two average curves which might be taken as a summing-up of the whole of the experiments.

Prof. Unwin.

Professor **W. C. Unwin** fully acknowledged the credit due to the Author for the labour he had expended on his experimental research, and for the high standard of accuracy and carefulness which he had aimed at throughout. There was no point of detail to which he had not paid attention, or in which any one could say that he had been careless in his observations. He hoped that in speaking on two or three points, in regard to which he differed somewhat from the Author's conclusions, it would not be thought that he differed in regard to the principal points that had been worked out, or with Major Cunningham's general conclusions. He should like first to call attention to one point in the experiments, which he thought explained why it was that the Author's measurements of discharge did not throw any great light on the relative value of the different formulas in use. The Ganges canal, as originally constructed, was in reaches of uniform slope, with a sudden drop at the end of the reach. It seemed to have been supposed by the constructors of the canal that the water would flow as it did in an ordinary stream of uniform depth, as at A A, Fig. 1, and that there would be a sudden drop at the fall. But it was well known that a sudden drop in a stream bed produced a form of water surface such as B B. The effect of an oversight, as it appeared to him, as to the influence of the sudden drop in the bed of the falls, was that the velocity in the canal was greater than was expected. At all events, the scouring action was greater, and that was corrected by building at the falls temporary weirs, which backed up the water and gave the water

Prof. Unwin. surface a shape like C C. At some of the sites where the largest number of experiments were made, Major Cunningham had placed himself at the point D of the canal, where obviously the surface-slope was most variable. The case was still further complicated by the fact that, in regulating the discharge of the canal, temporary obstructions were placed at the falls, and that, in low-water conditions of the canal, the obstruction at the fall was higher than the bed of the canal at the site of the gaugings. From one point of view that was extremely interesting. The Author had shown that the discharge under those conditions bore no relation to the depth of water in the canal, and he had thrown light in various ways on the action of water in a reach of that kind. But Professor Unwin considered that a site of that kind was not suitable for independent observations to verify the formulas of discharge; and the confession in the Paper that the measurement of the surface-slope was an exceedingly uncertain measurement, seemed to admit that the site of the gaugings was inconvenient for that particular purpose. It had been mentioned that the surface-slope had been measured in lengths of 4,000 feet and 2,000 feet, and that there was a difference of 25 per cent. Of course that threw great doubt on what slope ought to be taken in calculating discharges, by any formula which involved the use of the surface-slope. Gaugings were made over a length of 50 feet only, and it was still further uncertain whether the slope over 50-feet length was anything like the slope over 2,000-feet length. It seemed to him that the measurement of the surface-slope over a great length, when the gauging was over a very short length, was

Fig. 1.



Prof. Unwin.

not a satisfactory proceeding. If the surface-slope was measured by levelling operations, it was essential to measure over a considerable length. Of course a levelling instrument was not accurate enough to measure the surface-slope in a short distance; but it did not seem impossible to adopt micrometric methods in measuring the slope in a very short distance. It would be possible to have a baulk of timber, say 40 feet in length, of sufficient weight and solidity to hold a tolerably stable position in the water, on which might be placed a level with micrometric screw, so as to measure the slope, say in a length of 40 feet, to $\frac{1}{1000}$ inch. In that case it would be possible to get the surface-slope precisely at the site of the gauging.

All the experiments had been made with floats, and some discredit had been thrown on the use of current-meters for gauging. He did not say that one was better than the other, because there were cases in which floats must be used and current-meters could not be used; but, as he had lately had a good deal of experience in the use of current-meters, he would say a few words as to the relative value of the two kinds of instruments. The Author had enumerated several advantages of floats: 1st. "They interfere little with the natural motion of the water." That was true, and he would pass it by. 2nd, "they measure velocity directly." It was true that they measured the average velocity over a more or less considerable length of stream, but not at the section point; he did not think that was of much importance. 3rd, "they can be used in streams of any size." 4th, "they are not much affected by silt or floating weeds," which was quite true. 5th, "they measure forward-velocity," which was also true. 6th, "they can be made up and repaired by common workmen," which did not seem to be very important. And 7th, "they are very cheap." No doubt floats could be made very cheaply, but if, in the observation of velocity, they required two or three times the time which current-meters required, the instrument was not really a cheap one. In determining each velocity, the Author had taken forty-eight float observations, over a length of 50 feet; in other words, he had practically observed the average velocity over a length of 2,500 feet. He should show presently that with a much shorter length of stream current-meters gave practically a constant velocity. There were certain disadvantages in the use of floats. In the first place, it was impossible to get rid entirely of the action of the wind on the exposed surface of the floats. Although surface-floats were, of all others, the least open to objection, it was just the surface-velocity which would be the last that would

Prof. Unwin. be observed if velocity could be recorded at all points of the section with equal facility. The moment it was attempted to get velocity below the surface by the use of floats, difficulties were encountered which were considerable. Using what appeared to be the best instrument, according to Major Cunningham, the sub-surface float, with a very light surface-float, it was impossible to get rid of the action of the stream on the cord which connected the surface-float with the bottom-float, and that almost restricted the use of sub-surface floats, for anything like accurate measurements, to very moderate depths of water. When it was remembered that, in the Mississippi experiments, the cord connecting the surface- and the bottom-float had an area one and a half time as great as the area of the sub-surface float, it would be seen that, neglecting the influence of the cord must, in that case, have introduced an enormous amount of error. On the other hand, the current-meter, supposing it to be rightly fixed, and of one of the forms which obviated the objections to engaging and disengaging gear, certainly enabled velocities to be taken with more rapidity. He took some velocities in the tidal part of the Thames in January 1882, and on the average of five or six days' work, he obtained one good observation in about three minutes' work, a rate which he thought could not be approached in using floats. Then the velocity was obtained at the section at which it was wanted. Finally, it should be remembered that, with floats, unless the stream was comparatively narrow, there was the laborious operation of fixing the float-path by angular measure. There was one point with respect to which he thought there was a little misconception. Looking at the face of the current-meter, the blades of the screw appeared very large, and seemed as if they would interfere a good deal with the motion of the stream; but when the instrument was at work it was only the edges of the blades that met the stream, and he had a strong impression that the current-meter interfered very little with the natural action of the water, if only it was made with the right degree of delicacy. He had been using a current-meter which had a very convenient arrangement—not being fixed on any rod, but suspended by a wire. It was only open to one kind of objection, so far as he knew, that when the meter was suspended by a wire, it was uncertain whether it was exactly normal to the plane of the section, the mean velocity across which it was intended to measure; there was, therefore, a source of error in that kind of suspension, but it was as well to consider what magnitude that error was likely to assume. Suppose a meter to be fixed at 20° with the normal to the section the velocity measured would be erroneous by about 6 per cent. Now a meter suspended by a wire was not fixed at an angle with the plane of section, but it swayed backwards and forwards through a more or less large angle. Supposing it to sway backwards and forwards, through an angle of 40°, the error of observation due to its position would not exceed 3 per cent. That, however, he believed was a much greater angle than any through which the meter swung, and the error did not seem to be a large one; so that, at all events for many purposes, a meter suspended by a wire was an extremely convenient form of current-meter. With respect to the unsteadiness of the motion of water in a stream, he had used a meter during the last flood in the Thames in this way: he took a continuous note of the time of each hundred revolutions of the meter, and he had plotted in the three curves shown on the diagram (*post*, Plate 6, Fig. 8) the results of the observations taken in that way at depths of 0·5, 3, and 6 metres from the surface. Taking every hundred revolutions, there was an exceedingly irregular curve. The oscillations of the velocity during periods of twelve seconds for the two upper curves, and twenty seconds for the lower curve, were very large. But by simply averaging the result for five hundred revolutions of the meter instead of one hundred, there was an exceedingly regular curve. There were eleven or twelve successive periods during which the meter made five hundred revolutions. The velocity at the end of each interval was calculated from the five hundred revolutions, and a curve was obtained which approached closely to a straight line, and he was not sure that the irregularity which existed was not due to an imperfection in observing the time. He hoped to be able to repeat the observations with an electric chronograph, which would eliminate all error of that kind. One point in the Paper had not been alluded to—a point on which he entirely differed from the Author. There was a well-known phenomenon of flowing streams that the position of the line of maximum velocity was considerably below the surface, often at about one-third of the depth at the middle of the stream, more or less towards the sides. The explanation of that depression of the line of maximum velocity had been considerably discussed. The explanation adopted by the Author was, that the air opposed a resisting surface against which the water rubbed. The retardation due to the friction of the water against the air explained, he thought, the reduction of surface-velocity and therefore the depression of the line of maximum velocity. Although Professor Unwin did not deny that the air retarded the motion of the water, the explanation appeared to him to be an altogether inadequate one. The

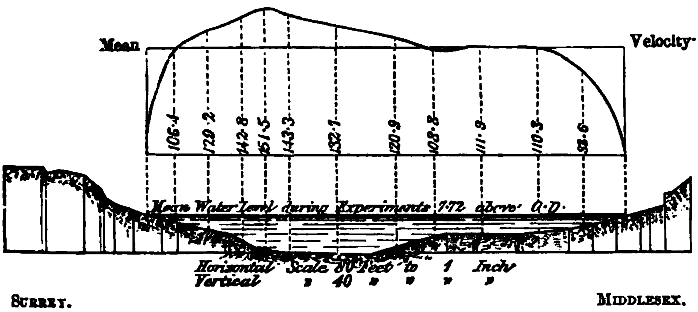
Prof. Unwin.

Prof. Unwin. only cause which seemed to him sufficient was the mixing with the surface water of water stilled by contact with the bed, and brought up to the surface. There was more than one way in which that occurred. Eddying masses of water produced against the roughnesses of the bed were shot off and mingled with the stream, and were liable to accumulate at the surface because it was a boundary of the section. Probably, in addition to that, even in a straight length of stream, a curvilinear motion of the water in spirals brought up the bottom water towards the surface; and furthermore at every bend in the river there was demonstrably a rotation of the water in the plane of the transverse section. Amongst the most interesting results mentioned were the Author's attempts to find some rapid way of approximating to the mean velocities in the section of the stream; and he had given two or three rules for finding the mean velocities of a stream, from observations made at two depths at the centre of the stream, or by one observation with a peculiar float which had a small surface float and two equal subsurface floats. In finding that means of rapid approximation the Author had proceeded entirely in one direction; he had tried to find the mean velocity from observations at two different depths. But Professor Unwin thought the mean velocity might be more easily found by observations at two positions in the horizontal width. To test this he had worked out as a rough trial two considerable sets of the Author's observations made with rod-floats, and he had found that if single observations had been taken with a rod-float at very nearly one-third of the breadth of the stream from the centre, he would have got approximately the mean velocity of the stream, and much more accurately if he had made two observations with a rod-float, at one-third the distance from the centre on each side of the centre line. He should like to mention one point which it appeared those who measured the flow of the water had been a little too apt to neglect. So far as he knew there were no observations on flowing streams in which the temperature of the water had been observed. He had found in experiments made in another way that, at the temperature of the atmosphere, about 60°, a very few degrees difference in temperature made a marked and measurable difference in the fluid friction; and he thought that in future it would be useful if experimenters would record the temperature of the water at the time their observations were made. He believed that some noticeable discrepancies in the results might perhaps be explained by observing the temperature. He had placed on the wall the results of two gaug-

Prof. Unwin. ings which he had made during the last year. One was a diagram of gaugings during the last Thames flood (*post*, Plate 6). He had drawn a curve of mean velocity which looked tolerably regular, and from it calculated the flood discharge. It was a little under 8,000 cubic feet per second—a result not very different from that which he had given to the Institution some years ago.[1] The flood was 6 inches lower than that of 1875, and at the section which he had chosen this year for gauging a large quantity of water, 3,000 or 4,000 cubic feet possibly, was escaping over some miles of flooded area on the bank of the river. He exhibited some results of velocity observations at a section of the river at Putney, in order to show how far it was possible, in a tidal stream in which the surface-slope and velocity varied from instant to instant, to get enough observations to calculate the discharge of the stream. The flow over Teddington Weir in the flood while the observations were being made was just under 71 cubic metres per second, or 2,500 feet per second.

Mr. Latham. **Mr. Baldwin Latham** exhibited a diagram, Fig. 2, showing the result of a number of gaugings which had recently been made in the River Thames, immediately below Teddington Lock. The gaugings were all made about the period of low water, after the tidal water had run off, and therefore represented the flow of upland water of the Thames at that point. At the place where the gaugings were made the river was slightly curved, the concave side of the curve being on the Middlesex side of the river, and the convex side on the Surrey side. The ordinates above the section

Fig. 2.



River Thames.

---

[1] Minutes of Proceedings Inst. C.E., vol. xlix., p. 156.

Mr. Latham. of the river represented the relative velocities at the points in the section of the river over which they were placed. The horizontal line represented the mean velocity of the stream. It would be seen that at one point there was a depression in the curve, showing that the velocity there was less than on each side. This occurred over a shelving bank on the concave side of the river; while, on a similar shelving bank on the convex side, there was an abnormally high velocity. There could be little doubt that these peculiarities were due to the horizontal deflection of the stream; for although the diagram was based upon upwards of 60,000 feet run of the current-meter, extending over numerous observations, the separate observations all more or less indicated this distinctive feature. With reference to the use of double-floats in gauging, no doubt under some circumstances they might be useful, especially in sluggish streams; but in the generality of streams the current-meter was undoubtedly preferable. Where the maximum velocity was removed one-third of the total depth of the stream from the surface, and where the velocity was represented by the ordinate at any point in the vertical section of a parabola having its axis parallel with the surface of the stream, double-floats were not necessary, as the surface-velocity in that particular case would be equal to the mean velocity, and so a single surface-float would give the true velocity of the stream. Judging from his own observations, he should say that it was very rare indeed for the maximum velocity to be removed so far as one-third the total depth of the stream from the surface. Where such a condition did exist, it was no doubt due to the vertical deflection of the stream downwards, arising either from the channel being deeper at the place of observation than higher up the stream, or from water being tailed back over the point of gauging. This, as suggested by Professor Unwin, might have been the case at certain points in the canal which had been selected as gauging stations. The effect of the damming back was to flatten the inclination in the lower portion of the longitudinal section of the canal, and the water entering from the upper portion at an angle, naturally was projected downward, and so the maximum velocity was removed from the surface. It was important to determine the position of maximum velocity. Looking at Professor Unwin's diagrams (*post*, Plate 6), it appeared that the mean velocity was not far from the centre of the stream; but in most channels of trapezoidal cross-section, in which the larger volume of water was moving with a relatively higher velocity, the point of mean velocity in the stream would rise above half the depth. As in rivers and streams of pretty constant flow

Mr. Latham.

there were two points in every section where the water would be moving at the mean velocity, if these two points were ascertained a ready means would be available for easily gauging, by a current-meter, the quantity of water passing. He had made numerous gaugings in this way, but he took care at the same time to check the records by an instrument which delineated every change in the area of the section. Great precautions were needed in arriving at results based on calculations in which the use of coefficients, and the rate of inclination of the surface of the water, were the principal factors. It was well known that within certain limits the length of the channel affected the velocity of flow. In long channels of uniform grade, as compared with shorter channels, there was an acceleration of flow; for example, the velocity of water in a 12-inch pipe, full or half full, having an inclination of 1 in 495, when the length was but 250 feet, would be 2 feet per second; but if the length were increased to 30,000 feet, the inclination remaining the same, the velocity would be 2¼ feet per second. He had found by actual measurement in the River Wandle that weeds attached to the bottom of the stream, and growing in the water-way of the channel, affected the flow of the water to an enormous extent; for while the ordinary Eytelwein formula in the Chézy form gave a coefficient of 93·4, in this case the coefficient, on an average of six carefully conducted experiments, fell to 40·23. This proved the enormous influence exercised by the growth of weeds in impeding the flow of water in a channel. With reference to the form of the curve at the surface of the water, there was no doubt that whenever a river was filling up, the surface of the water was convex, and that whenever the river was falling it was concave. If the stream remained constant there would be a pretty level surface. When a river was filling up two functions were in operation, the water was moving down the stream, and the channel was being filled. As water was moving down the stream the maximum flow was always in the deepest part, generally in the centre of the stream. The consequence was that the water fell from the centre towards the banks, but the reverse condition held when the stream was falling. In Mr. C. Ellet's work on "The Mississippi and Ohio Rivers," it was recorded (page 302) that whenever the latter river was rising the drift was thrown on the shores, clearly showing a current from the centre outwards; but whenever the river was falling, the boatmen said they could travel for miles in the centre of the stream without making a single sweep of the oar to keep them in the current. This amounted to an actual demonstration of the curves. The

Mr. Latham. Author of the Paper seemed to say that no results had been obtained from the observations upon silt. Mr. Latham presumed that that simply meant that a large quantity of silt was brought into the canal from day to day, and that the simple velocity of the stream itself had not much influence upon moving or creating the silt; but he wanted silt observations to be carried on from quite a different point of view. At Croydon there was a culvert 4 feet in diameter under the town. At certain periods it discharged beautifully clear spring chalk-water, at the rate of 3,000 to 4,000 cubic feet per minute. It was also connected with the street gullies of the town, and in time of rain discharged highly-coloured water carrying a large amount of silt. He had been making careful observations on the culvert for many years, and he had found that whenever the water was clear the maximum velocity, with the same height and gradient, was always considerably higher than when the water was turbid; thus the mixture of silt with water had the effect of retarding its velocity. He considered that the retardation in this case was due to the extra load the water had to carry; and if the weight of water and weight of silt were taken, and multiplied by the velocity, it would be found to tally pretty nearly with the weight of the water multiplied by its velocity when there was no silt. Thus by the addition of silt a diminution of velocity was effected, while in the absence of silt the velocity was greater. He contemplated making a series of observations with different percentages of material put into water, to ascertain to what extent the velocity of the water might diminish. However, it was obvious, from observations already made, that silt, when present in a large quantity, did interfere with the velocity of flow. The Author seemed much surprised that, with such a high temperature as 165° in the sun and 105° in the shade, there was only $\frac{1}{10}$ inch of evaporation per day from the surface water. For some years he had been carrying out experiments on evaporation. In former days it was considered that the evaporation in England greatly exceeded the rainfall, and there could be no doubt that the instruments used did show that such was the case; but common sense would show at once that if that were so there would be no streams. Mr. Charles Greaves, M. Inst. C.E., who had done so much to elucidate the question of evaporation, and other meteorological phenomena in which engineers were interested, had directed his attention to the views of Dalton on evaporation.[1] These tended

---

[1] Memoirs of the Literary and Philosophical Soc. of Manchester, vol. v., p. 574.

to show that evaporation was not due to temperature, but to the vapour-tension. Water at a particular temperature would give off vapour of a certain tension. The vapour in the air also had a certain tension which was arrived at by the temperature of the dew point. So long as the tension of the vapour in the air was less than the tension of vapour due to the temperature of the water, water would pass into the air, but when the tension of the vapour in the air was greater than the tension of vapour due to the temperature of the water no evaporation would take place. Although the Author of the Paper recorded no experiments with the wet and dry bulb thermometer, it was probable that at 165° and 105° the atmospheric vapour-tension would be in excess of the tension of vapour of water at 66°. No one could expect to blow steam from a steam-boiler under 10 lbs. pressure into a boiler which was under 20 lbs. pressure; but it would be easy to pass the steam from a boiler under 20 lbs. pressure into a boiler under 10 lbs. pressure. That was exactly what was found in evaporation. The month in which the largest amount of evaporation had occurred this year was May, yet the temperature of May was much below that of July. He would point out a source of serious error in all the experiments on evaporation which he had tested. He had adopted as the standard form of evaporator the form introduced by Mr. Greaves, namely, a floating evaporator, but he originally started with one similar to that described by the Author. He had a vessel floating in water. It was supported by an outside ring, which came above the water, but he found that when the sun in hot weather shone upon the evaporator, as the water always ran up the side of the vessel by capillary action, great evaporation occurred. The error in all such vessels was no doubt due to the film of water that ran up the sides, and which under great heat was speedily evaporated. He therefore discarded that form of evaporator, and suspended the evaporating vessel from a floating ring like a life-buoy, and at once there was a large reduction in the amount of evaporation. The film of water now extended up the sides of the vessel both on the outside as well as the inside, and so they were kept cool. He, however, carried the experiment still further on a number of evaporators painted different colours. He observed that from a painted gauge, even if black, there was a less amount of evaporation than from a plain copper gauge. That was because capillary action was more energetic in the metal gauge than in the painted gauges. In order to test the matter, he took three evaporators made of copper, each 5 inches in diameter, and holding at least 1 foot depth of Mr. Latham.

Mr. Latham. water. One of these he had slightly greased inside, and that and another copper evaporator were allowed to stand in a tank of water immersed to within 2½ inches of the top, while at the same time the third evaporator was freely exposed to the atmosphere. The evaporation from these gauges in the month of May, 1882, when compared with that of the floating gauge 12 inches in diameter, and a gauge painted white, but immersed in water, were—

Floating Gauge.	Gauge in Air.	Gauge in Water.	Gauge Greased in Water.	Gauge painted white in water.
Inches.	Inches.	Inches.	Inches.	Inches.
3·665	6·355	5·495	4·085	4·175

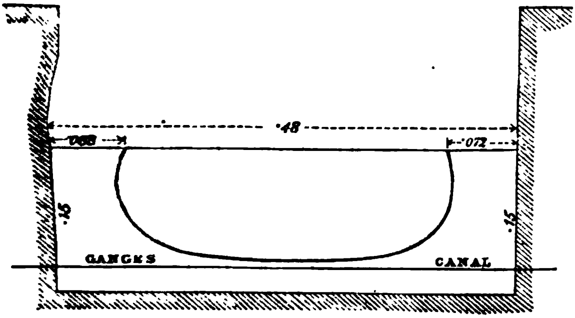
In the case of the gauge greased regularly, the coating was so slight that no oily matter ever appeared on the surface of the water. The size of the instrument had a marked influence on the amount of water evaporated, for the larger the vessel, in proportion to the area, the smaller was the marginal ring up which the water passed by capillarity, and from which it was evaporated.

Mr. Newman. Mr. F. Newman, in reference to the Author's contention that Bazin's two coefficients (β, C) were not reliable, said he ought at the same time to have stated that the conditions under which the formulas were obtained, which he now brought before the Institution, were very different from those of Darcy and Bazin. The average width of the section of canal was 180 feet to 200 feet, and the depth in the centre was only about 9 feet or 10 feet. The ratios of the depth to the width in the experiments made by the Author were as one to twenty, while those of Darcy and Bazin were much less. In Fig. 3 was represented exactly the proportion that the depth bore to the width in the Author's experiments. Attempts to reach the coefficient for velocity in a section in this must differ very much from one in the other on account of the greater excess of the wetted perimeter in the case of the canal. The section of the channel, as shown by the Author, did not fairly represent the canal as it was actually constructed. That would represent a depth of 20 feet. With regard to the mean velocity the curve shown was Darcy's; if a horizontal line were drawn, which unfortunately he had not shown on the diagram, at one half the depth of the water, and a point were taken on the surface one-third of the half width from the border, and a vertical line drawn from that point, the intersection of those two lines would be a point of mean velocity. This nearly coincided with what was stated by Professor Unwin; in fact, the point 3·5

Mr. Newman.

or 3·6 on the surface would represent the mean velocity as stated by him. The Author also objected to certain formulas formed by Professor Moseley, and by Darcy and Bazin, and also to some experiments by Darcy on pipes; but those on the pipes were remarkably correct, and they had been verified at Glasgow. Mr. J M. Gale, M. Inst. C.E., gave the figures, the area of the pipe, and the rate of declivity, to the late Professor Rankine, who worked them out, and the slight difference between the coefficient of friction, as deduced by Gale on 4-feet pipes, and that given by Darcy, amounted to less than one-thousandth per cent. That spoke well for the accuracy of Darcy and Bazin's experiments. Still he thought that the Author's remark to which he had referred should not have been made, because Bazin's coefficients were not applicable to a case of such enormous width in comparison with the area.

FIG. 3.



Mr. Vernon-Harcourt.

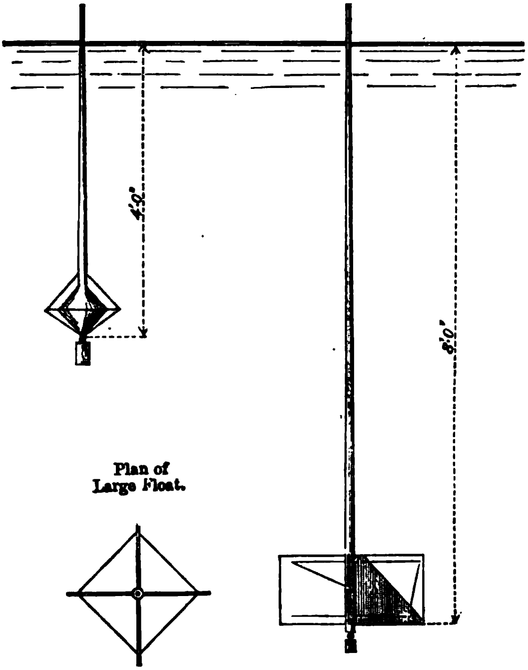
Mr. L. F. Vernon-Harcourt said that observers of discharge had in most cases adopted either the double-float or the current-meter, and had generally considered that one or the other was exclusively the best. For instance, in Humphrey and Abbot's experiments on the Mississippi the double-float was adopted to the exclusion of the current-meter. Mr. Révy, on the contrary, held that the double-float was perfectly useless, and used current-meters alone for measuring the discharge of the Paranà and La Plata rivers. Then, again, Mr. Gordon, in his experiments on the Irrawaddy, preferred the double-float, and the Author had followed his example and rejected current-meters as possessing insurmountable difficulties in application, whereas Professor Unwin had carried out experiments satisfactorily with a current-meter. These facts pointed to the conclusion, which he had found in

Mr. Vernon-Harcourt. practice to be correct, that both instruments were suitable for measuring the discharge, and it was necessary to find out which was best adapted to the particular case. Of course sometimes the double-float was the best, as, for instance, where the stream was sluggish and the channel fairly regular; but where the channel was irregular, where there were weeds not merely floating along the stream but growing in it, and where the bottom was uneven, the current-meter was far preferable. But there was another objection to the double-float, which the Author had indeed mentioned, but passed over without the notice it deserved, namely, the effect of wind on the surface of water. According to the Author the primary effect of wind appeared to be the production of wave-motion, that it caused translation of the water only when long continued, and that, were it not so, every wind would produce a current on a lake or at sea. Of course it was well known that long-continued winds did produce currents, as the trade winds created currents in the Atlantic Ocean. There were also currents on the Suez canal, at one time in one direction, and at another time in another. These were produced by the prevalent winds in the Mediterranean and the Red Sea. Perhaps these would come under what the Author would term winds of long duration; but if a strong wind was blowing up the Thames for even a short time it affected the tide. The Author also said that the velocity at the surface of a stream was affected by the resistance of the air. If, then, a long-continued wind could produce a current, and, moreover, the resistance merely of the air affected the velocity of a stream, wind, even for a short time, must have some effect. This was brought to his notice in some float experiments he made in 1875 on the Firth of Clyde. In that case he had to measure how far the ebbing current would take down any floating material. He had a float made, which was especially designed to provide against the action of wind. The experiments were conducted at the best time of the year, in June and July, when there was no great amount of wind. The float was a tin tube above, and lower down it was brought out in the shape of a cone, hollow inside. To make it float upright he had a weight attached underneath. It was about 4 feet long, and after a time he added some wings, with the object of offering a greater area to the tide where he thought that the wind would have no effect; but on a day when there was only a moderate amount of wind, blowing upstream, he found that instead of going down with the ebb tide it travelled in the opposite direction, though it reached about 4 feet below the surface (Fig. 4). At

Mr. Vernon-Harcourt.

the same time an orange travelled up against the tide very much quicker. He concluded that the wind evidently did affect water at the depth of 4 feet, and therefore he had another float made, consisting of a tin tube, carried down about 8 feet. At the bottom there were four tin plates, arranged radially to the tube. He braced these with a little tin piping so as to keep them secure (Fig. 4). He had only to add a very small weight of lead to keep it floating vertically in the stream. This float travelled down with the ebb when there was a strong wind blowing upstream, and when, of course, an orange would have been practically worthless. During the course of his experiments on the Firth of Clyde, he, several times, on calm days, started his two floats (Fig. 4) and an orange, as a surface-float, together, and observed that, whilst the calm continued, the three floats travelled fairly together, the surface-float generally going rather in advance, and the deep float falling somewhat behind. Directly, however, any opposing wind sprang up, the motion of the floats was reversed,

FIG. 4.



Scale ⅜ inch = 1 foot.

Mr. Vernon-Harcourt. the deep float taking the foremost place, and the orange coming last in order. He had often wished that he had the means and opportunity of making experiments of this kind on the surface of a lake where there was no natural current, and where the effect of wind upon the surface of the water might be tried. This was an important subject, and one that had not received due consideration. He was certain it must materially affect the experiments with double floats.

The reason advanced by Professor Unwin for the surface-velocity being less than the velocity some distance below, was that when there was an irregular bottom, the water striking against the shoals was checked, and being forced upwards imparted its loss of velocity to the surface. He could not accept this, because it seemed to him that the lowest layers of water, being checked, must impart their loss of velocity to the lower portions first. He did not see any reason why the lowest stratum of water should arrest the motion at the surface more than it did those layers nearer the bottom.

The Author evidently thought that the only formulas that could be satisfactorily got from his experiments were those in which the mean velocity could be expressed in terms of maximum velocity; but, unfortunately, a formula of this kind, though very useful on rivers and canals for determining the discharge, would be quite useless in determining what the discharge would be after a channel had been enlarged or upon a new canal. Therefore, in such a case as that, some kind of formula was required other than one which merely dealt with the mean velocity. He quite admitted that an experiment in an existing stream, from which the mean velocity was got, was much better than anything computed from a mere formula; but he would be very glad if the Author could, by means of the valuable experiments he had made, deduce some formula that would give the discharge in terms of the slope, because that was the only way in which a formula could be useful for rivers that had been enlarged, or for new canals.

Mr. Moore. Mr. B. T. Moore said the Author stated in his Paper that the main object of all his hydraulic experiments was the determination of the discharge of large canals and rivers. The quantity of water which passed down a stream in a given time was equal in volume to the solid contained between a transverse section of the stream, a portion of the bed, a portion of the free surface, and a certain surface extending from the free surface to the bed and from bank to bank. This last surface was of a curved form, and generally convex at every point down the stream, whether it was sup-

Mr. Moore.

posed to be cut by horizontal or by vertical planes. The only practical way of measuring a quantity of that sort was by obtaining a sufficient number of parallel sections made by planes perpendicular to the transverse section, and equi-distant from each other. When the areas of these sections, and their common distance were known, the prismoidal or other formulas of approximation would give the area to any required degree of exactness. This was the method he had always used when gauging rivers, and it was also the method which the Author had adopted.[1] But the areas of the parallel sections could only be found by means of velocity-measurement, and hence it was that the velocity-measurement was such an important matter. The Author took a number of velocities at short distances from the surface towards the bottom, (generally about 1 foot) from which he plotted a curve, and found its area, making allowance for that portion of the curve which was below the lowest velocity and above the bed of the stream. But the Author himself soon found that that was a very tedious and troublesome business; and, moreover, as it was of extreme importance that all velocities should be taken as nearly as possible at the same time, it became necessary to find some quicker means of doing the work. He was thus driven to seek some formula which would give the mean velocity at any vertical from two observations of velocity, one above and one below the half depth. Now, how did he arrive at that formula? He assumed that the form of the vertical velocity-curve was a portion of a parabola with its axis horizontal. If that assumption were made there was no necessity for a single practical experiment. By a simple mathematical transformation an equation could be obtained which would give the quantity sought. That equation was the following:—

$$4\,(3\,\theta^2 - 3\,\theta + 1)\,U = V_{\theta h} + 3\,(2\,\theta - 1)^2\,V_{\frac{h}{3}\,\frac{3\theta-2}{2\theta-1}} \quad . \qquad \text{(A)}$$

in which h was the depth of water, and θ a fraction which might have any value not exceeding $\frac{1}{3}$; U was the mean velocity, and the two measured velocities were represented by the letter V, one being taken at the depth $\theta\,h$ below the surface, and the other at the depth $\frac{h}{3} \cdot \frac{3\,\theta - 2}{2\,\theta - 1}$.

If $\theta = 0$, equation (A) became

$$4\,U = V_0 + 3\,V_{\frac{2}{3}h} \quad . \; . \; . \; . \; . \; . \; . \; . \; . \; . \qquad \text{(B)}$$

---

[1] Its application to the flow of the Thames had been given in the Minutes of Proceedings Inst. C.E., vol. xlv., p. 237.

Mr. Moore. This was the principal formula mentioned by the Author, and gave the mean velocity in terms of the surface velocity, and the velocity at two-thirds of the depth.

Again, if the quantity $3\,(2\,\theta - 1)^2 = 1$, so as to make the coefficients of the two measured velocities the same, the resulting equation was

$$2\,U = V_{0\cdot211\,h} + V_{0\cdot789\,h} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad \text{(C)}$$

This was the last formula at which the Author said he had arrived, his object in seeking it being to obtain a formula which should be applicable to a double submerged float, and should give the mean velocity from one observation. This last formula was not convenient for rapid work in the field, where the depth at which a velocity had to be taken has so often to be calculated mentally, and it was of no particular value for any other instrument except the double submerged float.

Another very convenient formula might be obtained from the general formula (A) by giving θ the value $\frac{1}{12}$. This formula was

$$37\,U = 12\,V_{\frac{1}{12}h} + 25\,V_{\frac{7}{10}h} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad \text{(D)}$$

and an approximate form of it, good enough in many observations, was

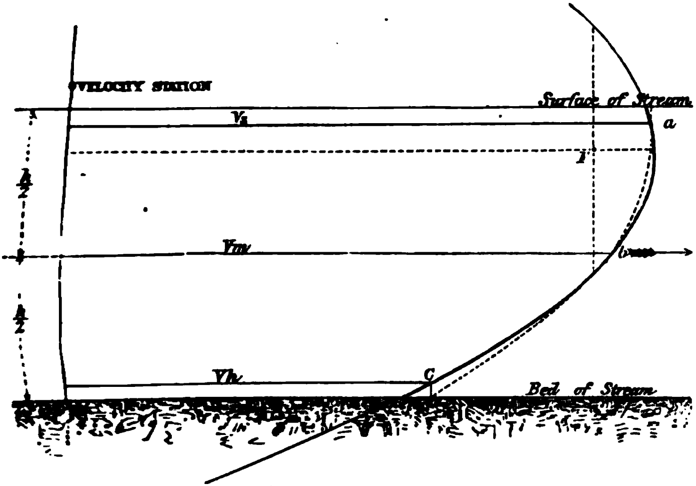
$$3\,U = V_{\frac{1}{12}h} + 2\,V_{\frac{7}{10}h}.$$

The quantities $\frac{1}{12}\,h$ and $\frac{7}{10}\,h$ were easily calculated mentally, and they were the only quantities required to be known during the actual measurement of the velocities.

The formulas (B) (C) and (D) could all be used with current-meters as well as with floats, but the last was the most convenient, as it avoided surface velocities, and was very easy of application. But it was not every observer, or every experimenter, who accepted the theory that the velocity-curve was a parabola with its axis horizontal. On the contrary, some of the best experiments tended to show that the curve was better represented by a parabola, with its axis vertical, and its vertex in the bed, at least as far up as the maximum velocity. If that was assumed to be the case, it would result in an entirely different formula. It was quite possible that the two formulas might give the same, or nearly the same, mean velocity. Considering that there were two rival theories as to the form of the velocity-curve, and that they gave two totally different formulas, he thought great caution should be observed in adopting any formula depending on two measured velocities, and that it was safer to trust to a formula based on three velocities, which would always give the area of the velocity-curve to a close degree of approximation whatever the form of the curve might be. Mr. Moore.

Fig. 5 would render the subject clearer. The curve shown was a parabola with its axis horizontal and focus at F. Whatever the true form of the velocity-curve might be, it was always curved in one direction, and was generally much flatter than the curve shown. Now as the radius of curvature of a parabola varied from half the latus-rectum to infinity, and as the latus-rectum might have any value, a parabola could always be found which should pass through three points of the velocity-curve, and should coincide with it very closely throughout. The area of this parabola was easily calculated from three known velocities, and it could only differ very slightly from the area of the true velocity-curve. In Fig. 5 the three lines marked V_s, V_m, and V_h represented the three measured velocities, V_s being taken at the small distance δ below the surface, V_h at the same distance above the bed, and V_m at half the depth.

FIG. 5.



Vertical Velocity-Curve.

The area between the velocity-station and the curve was then given by the formula—

$$\text{Area} = \frac{h - 2\,\delta}{6}\,\{V_s + V_h + 4\,V_m\} + V_s\,\delta + V_h\,\delta \quad . \quad (1)$$

Mr. Moore. if the two exceedingly small triangles at *a* and *c* were neglected. But in practice it was seldom necessary to take into account the small distance δ. It was generally sufficiently accurate to consider V_s the velocity at the surface, and V_h the velocity at the bottom. The formula for the area thus becomes—

$$\text{Area} = \frac{h}{6}\{V_s + V_h + 4\,V_m\} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

The difference between (1) and (2) was

$$\frac{2}{3}\,\delta\,(V_s + V_h - 2\,V_m).$$

Now as δ was a small quantity, and as the velocity at mid-depth never differed much from the arithmetic mean of the velocities at the surface and bottom, the error made by using formula (2) in place of formula (1) was always very small.[1]

The same conclusion would follow from an inspection of Fig. 5, in which the dotted line represented the curve resulting from the supposition that V_s and V_h were the surface and bottom velocities. It would be seen at once that the area bounded by the dotted curve was too small above the half depth and too large below it, and that on the whole it could only differ from the area in question very slightly. In general the velocity-curve was much flatter than the curve drawn, which was purposely much curved for the sake of giving distinctness to all parts of the figure.

So far he had spoken only of the use which was to be made of these three velocities and the parallel sections, but now he would consider how these velocities were to be obtained, for that was a most important thing. The Author put his faith entirely in floats, not having a good word to say for any current-meter. It was not his intention to discuss all the objections that were raised to current-meters, but he would make this remark, that current-meters were indispensable and floats were not. Some years ago he had measured the discharge of the Nile. He had to do that at low Nile, which took place about the 21st of June. All the velocities had to be taken in three days, because the river

---

[1] In a velocity-curve taken on the Thames the following numbers were found:—

$V_s = 160{\cdot}4$ feet per minute.
$V_m = 149{\cdot}6$ " "
$V_h = 135{\cdot}9$ " "

and δ was 6 inches.

$\therefore$ difference above $= \frac{2}{3} \times \frac{1}{2}\,(160{\cdot}4 + 135{\cdot}9 - 2 \times 149{\cdot}6) = 1{\cdot}27$ square feet, the whole area being 244 square feet.

remained pretty well constant during that time but no longer. Mr. Moore. Now, even at low Nile the best site which he could select was 1,600 feet or 1,700 feet in width, with a mean depth of over 10 feet. There were four sections extending to ½ mile, nearly equidistant from each other. The method he adopted was to measure the discharge through each of those sections, and compare them together as checks upon each other. There were sixteen principal velocity-stations in each section. That was the smallest number with which he could operate on such a large scale. That meant two hundred velocities, and all these two hundred velocities had to be taken within three days. Egypt, in June, was not a country in which one could work late in the day. All the work had to be done before ten o'clock in the morning, after which time the heat of the sun put a stop to fieldwork. He would ask how the work could have been done by floats? As to taking velocities at depths where a number of observations had to be taken to get one velocity, it would have been simply out of the question. That was an instance where current-meters were absolutely essential if work had to be done in a given time. The Author had stated that floats were cheap, and current-meters expensive. But were floats cheap? If they were used, a number of skilled observers were necessary, and such observers were by no means cheap. The expense incurred in this way would soon pay for a considerable number of current-meters. One person could use three current-meters at the same time without the slightest difficulty. He could put one near the bottom and one near the surface, and a third in the middle, and get all the observations at the same time. A great saving of time was thereby secured, and the velocities were taken together, which was also an important point. He was speaking now of using the current-meter by means of suspension. Any contrivance for fixing the current-meters in position was a cause of great difficulty and delay. The Author objected to current-meters in suspension, and spoke of the uncertainty of orientation. But Professor Unwin had pointed out that there was not much in that objection about orientation, and that if a current-meter was set at 20° from the direction of the current the error in its registration would not exceed 6 per cent. of the velocity, and if allowed to swing 20° on each side of the true direction the error would not be more than 3 per cent. Mr. Moore had used suspended current-meters very freely, but had never seen one swing 20° from the normal on each side; 10° would be an ample allowance to make. But with an allowance of 10° there was little to be said on account of this defect in orientation, Mr. Moore. because the error would not amount to more than $\frac{3}{4}$ per cent. The Author also objected to current-meters being suspended, because there might be an error in position as regarded depth; but if the stream was a slow one, the suspending line was very nearly vertical, and there was no question of error in the position of the meter. If the stream was rapid, of course the line, even when the current-meter was loaded, would be drawn back a little from the vertical, but in that case there was a compensation. The variation of velocity from point to point vertically was very small compared with the velocity, when the velocity was considerable, and therefore it was not of so much importance whether the meter was a little above or below the level where it was intended to be. In his book the Author showed a current-meter fixed to an apparatus attached to two pontoons; the current meter was deprived of its tail, and was held in a position of restraint parallel to what the Author supposed to be the axis of the stream; but if the current-meter was allowed to hang freely, it would find out the axis of the stream for itself, much better than could be done from the surface. The Author raised several other objections to current-meters, which he could not accept, but he would not discuss them, because he had already taken up too much time. In his opinion the best way to obtain the mean velocity, and from it the area of a vertical velocity-curve was to use a current-meter which admitted of being lowered from the surface to the bottom with tolerably uniform velocity, and also of being drawn up again with uniform velocity. It was not necessary that the velocity going down should be the same as the velocity coming up. The whole reading, divided by the time, gave the mean velocity; nothing could be simpler. The mean velocity was got by one reading, and the elaborate contrivance of the electrical bell and apparatus was got rid of. He did not like those arrangements, because perhaps just at the very critical moment something might go wrong with the insulation, or the battery might not work, and so the current-meter would be rendered useless, or would give incorrect results.

Mr. Cowper. Mr. E. A. Cowper wished to make one or two remarks in reference to the question raised in the Paper on the instability of the floating gauges. Balls $1\frac{5}{8}$ inch in diameter, were spoken of, weighing but little more than the water displaced. They were kept from sinking by a thread and small float at the top. It seemed to him that it would be advisable to have a larger float. He would prefer to have two boards each 1 foot square, put across each other for the lower float, or at all events a large surface on

Mr. Cowper.

which the water could act. But the point to which he wished to draw particular attention was this, that if the water on the surface went faster than the water below, the top float drew the string aside horizontally, and lifted the lower gauging board higher than it ought to be, as pointed out in the Paper. If, on the contrary, there were three threads hanging from three points at a distance apart on the top float, the lower float would always be at the proper depth from the level of the water, unless the current was so strong as to slack the threads altogether. It seemed to him that a triangular top float formed of three light flat bars of wood, to take the three threads to suspend the large bottom float, would ensure the exact depth of the lower float below the surface of the water. He wished to ask what price was charged for the water for irrigation? He understood that the experiments were all for the purpose of finding out what quantity of water to charge for, but the price was not stated, nor how the water was measured and sold to the natives.

Sir Frederick Bramwell.

Sir **Frederick Bramwell** said three points had been touched upon by the previous speakers, about which he wished to say a few words. One was as to the effect of wind on the surface of the water—whether it was merely skin deep, or was effective in moving the water along. Recently he was at Niagara, and when crossing below the Falls in the ferry boat, he asked the boatmen whether there was much variation in the height of the river. Their answer was that it depended not so much upon the season as on the direction of the wind. He crossed on several occasions, and one day the boatman pointed out to him that the water in the river below the Falls had risen 12 inches, entirely in consequence of a change of wind acting on the water above. That was good evidence that the direction of the wind either retarded or augmented the flow of the water. He therefore thought it must be regarded as effective far below the surface. Mr. Baldwin Latham objected to the possibility of there being a greater evaporation than was equal to the rainfall. It appeared to him, however, that all the rivers on the face of the earth must be derived from the excess of evaporation from the sea over the rainfall on the sea. The last speaker had suggested that if a current-meter had not got its tail, and was not allowed to follow the direction of the stream for itself, it would give an inaccurate result; but was that so? Was it not desirable that the current-meter should be fixed in a line in the axis of the stream, if it was required to know, not what was the maximum flow of water in some one direction at that particular point, but what was the flow of water in the

Sir Frederick Bramwell. direction of the river? It was quite clear that there were eddies all through the river. If there were not, the flowing river would deposit the suspended matters, just as they were deposited in a quiescent lake. What was wanted, therefore, was not that the current-meter should seek out for itself the point of strongest current, and should sway backwards and forwards as the current varied, but that the meter, always pointing in one direction, should give the velocity in that direction from time to time, because that when multiplied into the cross-section would give the true velocity, and not the velocity that would be attained if the meter were at liberty to wander into the line of greatest current at the time being.

Mr. Redman. Mr. J. B. REDMAN directed attention to the well-known treatise by the late Major Rennell on ocean currents traced by him to the winds. Amongst others a local one at the back of the Goodwin was quoted, which increased in intensity after a continuance of S.W. gales. The recent abnormal addition to the tidal column of the Thames, an increase of 5 feet, or 25 per cent. as regarded altitude, due almost entirely to gales of wind, must necessarily have been accompanied by a corresponding increase of velocity.

Mr. Latham. Mr. BALDWIN LATHAM said that the amount of evaporation from the sea was equal to the amount of rain falling on the sea and the volume of water carried by streams from the land into the sea. It was obvious that, taking equal areas, the depth of evaporation from the sea was less than the depth of the rainfall on the land, as the quantity of water carried by rivers and streams into the sea from the land was less than the rain which fell on the land. The excess of depth of rainfall on the land arose from the smaller area of land compared to that of sea, evaporation also taking place from the land as well as from the sea, which vapour, on condensation, fell again on the land in the form of rain or dew.

Major Cunningham. Major ALLAN CUNNINGHAM observed, in reply to criticisms as to the suitability of the Ganges Canal for experiments on the flow of water, in consequence of its being laid out in reaches closed by obstructed falls, first, that irrigation canals in hot climates were of such enormous importance, both financially and in the interests of humanity, where the very existence of immense populations sometimes depended on them in seasons of drought, that the conditions of the flow of water in them, even if peculiar, were of more public importance than those of flow of water in channels wholly natural. But the condition in question, of obstruction at the tail of each reach, was in no way peculiar; it was common to most fresh-water canals and also to most rivers of small size in highly civilised countries.

Major Cunningham.

A more important objection had been raised by Professor Unwin as to the position of the two sites,[1] at which most of the experiments were made, as being within the influence of the obstruction at the tail of the reach. The reach was 9½ miles long, and the sites in question were 5½ and 4¾ miles from the obstruction; the fall of the bed was 11·5, 6·1, and 4·9 feet in those lengths, whilst the raised crest of the falls at the tail of the reach was 5·1 feet high, or 0·2 foot above the bed of the lower site; but there were also means of raising the crest-level temporarily about 4 feet more, or over 4 feet above the bed of the lower site. This might no doubt be an objection to the position of both sites; but apart from that they were exceptionally favourable for experiment, from their situation in a straight 3-mile length of channel with regular masonry banks, an advantage which could not have been secured elsewhere. The power of varying the obstruction at the lower site was the source of, perhaps, the most important feature of the experiments, namely, the great range of conditions and of consequent results; for example, surface-slope 480 to 24 per million; velocity, 7·7 feet to 0·6 foot per second, &c., which were obtained at a single site, and in some cases[2] without change of depth at the site. In no other large experiments had this great range been approached, nor could it be approached probably without artificial regulation; whilst without a wide range it was not likely that general laws of fluid motion would be discovered, nor a single formula be properly tested.

It was suggested that surface-slope should, if possible, be deduced from the surface-fall in the same length, say 50 feet, as the run through which the floats were timed. The "slope-length" should certainly be the shortest practicable, but it would be impossible to measure the fall of the free surface in a 50-feet length in any ordinary stream. The oscillations of the free surface, often ½ inch, and the awkward position of the body and eye, necessarily above that surface, precluded determining the minute quantity in question, say ⅛ inch in 50 feet, with accuracy. The introduction, as proposed, of a baulk of timber 50 feet long into the running water for carrying micrometers would ruffle the surface. The only hopeful way of doing it appeared to be to lead the water from two fine nozzles 50 feet apart, similar, and similarly placed, into two still-water boxes, the difference of water-level in which

---

[1] Soláni Embankment main site, and Soláni Twin Aqueduct sites.

[2] P. 26 above.

Major Cunningham. could be accurately read with a differential[1] hook-gauge or differential Pitot's tube; but no doubt the result would be affected by any want of similarity in the nozzles or in their position.

The objection to floats, that though they were cheap their use was expensive from the frequent repetition, and therefore great length of time required to determine properly an "average velocity," was undoubtedly an important one. If it was true that an "average velocity" could be determined, as apparently suggested by Professor Unwin, in three minutes with a current-meter, then the current-meter should, if certain improvements could be effected in it, supersede the use of floats in suitable channels. The drawbacks to the current-meter were discussed at length in Chapter XXIII. of the Roorkee experiments. The objection that floats, even when deeply submerged, were seriously affected by wind, was contrary to most recorded experience. The instance given by Mr. Vernon-Harcourt of a large sub-float with 4-feet immersion being driven by a high wind against tide was extraordinary; the ascribing this effect to the wind was contrary to all the Roorkee experience. Out of many hundred cases of upstream wind, not a single instance was noticed of even a surface-float (much less of a sunken float) being driven upstream by the wind: but the instance in question was in a tidal channel; in such a channel there were often counter subcurrents at certain states of the tide, so that this instance was not certain evidence of wind effect.

The double-float proposed by Mr. Cowper had two great defects; the large triangular surface-float was much too large, and the three-cord connector exposed much more resistance to the current than a single cord of the same strength would; these were the two worst faults in a double-float.

The statement that in the Mississippi experiments the double-float was so badly designed that its connector exposed one-and-a-half time the area of the sub-float to the current depended probably on the description given in the original Mississippi report, in which the connector was said to have been $\frac{2}{10}$ inch thick; this had since been reported[2] to be a misprint for $\frac{1}{10}$ inch, which reduced the ratio to 8 : 10.

Attention had been drawn by Mr. Baldwin Latham to some properties of transverse velocity curves in his own experiments, viz. (1) the maximum velocity being over the deepest channel; (2)

---

[1] Suggested by Mr. Rogers Field, M. Inst. C.E. Its use would be difficult in a stream liable to much rise and fall.

[2] Chief of Engineer's Report, U.S.A., for 1875, p. 114.

Major Cunningham.

the existence of two maxima on each side of a central shallow; (3) a depression in the curve near a shoulder in the bed; all these points were obvious in the Roorkee experiments. (Vol. I., pp. 257-260, and Pl. xxxv., xxxvi.) The statement of the maximum and mean velocity past a vertical being at definite depths, viz. at $\frac{1}{3}$ and at $\frac{2}{3}$ of the full depth did not agree with experiment; the maximum velocity might lie anywhere between the surface and mid-depth, and the mean velocity line varied in position with it, but the curve was so flat that the velocity at $\frac{5}{8}$-depth was, except near the banks, an approximation to the mean. The great variability of the coefficient (C) in the Chézy formula ($V = C \times 100 \sqrt{R S}$) was the very point towards which most of the Darcy-Bazin and Roorkee experiments were directed.

The accuracy of the Darcy-Bazin experiments on which so much stress had been laid had never been questioned. The suggestion of Mr. Newman that the failure of their coefficients when applied to the Roorkee results was due to the disparity of proportions of the Darcy-Bazin canals and the Ganges canal was very likely correct, and amounted to an admission of the want of generality of those coefficients, as urged in the Paper.

One main result of the Roorkee experiments was that approximation to mean velocity was more likely to be attained by direct velocity-measurement than by surface-slope measurement (p. 31); but it could not be fairly said that scarcely any light was thrown on the use of formulas involving the latter. Much special experiment was done (pp. 28–30) with this very aim, and with the definite result that Kutter's formula was the only one (not requiring velocity-measurement) of pretty general applicability, and would under favourable conditions give results differing by not more than 7½ per cent. from actual velocity-measurements. This was surely a definite and important result.

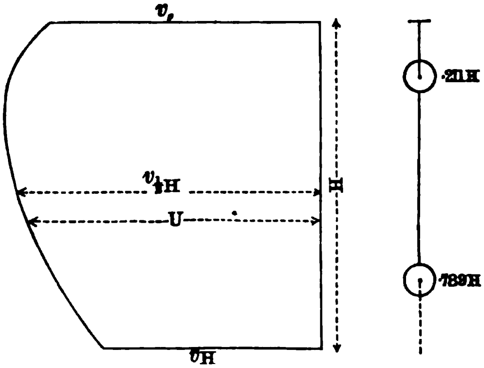
The process used by Mr. Moore for discharge-computation was substantially that used in the Roorkee experiments. The velocity-formulas given by him were mostly those of, or easily deducible from, that work (pp. 211–213, vol. I.) The spacing at 0·211 and 0·789 of the full depth, Fig. 6, approximately at $\frac{2}{10}$ H and $\frac{8}{10}$ H, which were simple fractions, had great advantages. It gave the best general approximation possible, with only two ordinates, for any curve whatever.[1] The two velocities could be measured at one operation, which saved time, with a special double-float. When the two velocities were separately measured, the result was

---

[1] Todhunter's "Laplace's, Lamé's, &c., Functions," pp. 122, 126.

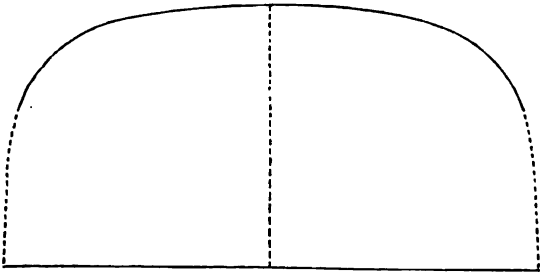
Major Cunningham. at once found as the arithmetic mean, which saves computation. Next, it was hardly fair to describe the taking the vertical curve to be a parabola with horizontal axis as a mere assumption. The curve was most likely not a parabola, but the amount of evidence that it did approximate very nearly to a parabola with horizontal axis, sufficiently for computing approximations, was now very great (chap. xi., vol. I.). The parabola was by no means so accommodating a curve as represented. Thus, assuming a horizontal axis (Fig. 6), it could only be fitted to three points, whilst forty-five of the Roorkee curves had more than three, and ten of them have ten measured ordinates. This amounted to a

FIG. 6.

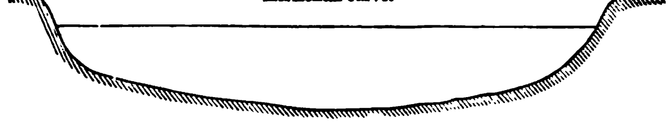


Vertical curve.

FIG. 7.



Horizontal curve.



CROSS-SECTION OF CHANNEL.

Major Cunningham.

severe test. On the other hand, the placing the axis vertical was certainly wrong, being incompatible with a maximum velocity-line below the surface, a condition the evidence of which was overwhelming.

The only evidence adduced by Mr. Latham as to the convexity or concavity of a river when rising or falling was very indirect; the observed facts might well be due to other causes; for instance, the set of driftwood towards the banks might be due to wind or to surface-currents.

As to evaporation, dryness of the air was probably the most important exciting cause. In northern India the dryness of the air during the hot winds was excessive, the difference between dry and wet bulb thermometers being often 40°; so that evaporation must then be active. The smallness of the evaporation from the canal at Roorkee seemed to be due to the coldness of the water. The use of oil inside an evapometer would be hardly safe, as a little oil would probably escape and form a surface skin over the water, and so reduce the evaporation.

Correspondence.

Mr. A. Flamant.

Mr. A. Flamant considered the Author's experiments to be extremely important, and that his book,[1] full of practical details of the manner in which he worked, should be read by all those who proposed to undertake similar experiments, or who desired to study the phenomena, still very obscure, which occurred in the flow of water in large open channels. It was for this reason that he had referred to the work in a note inserted in the "Annales des Ponts et Chaussées,"[2] desiring thereby to induce French students of the subject to consult Major Cunningham's book. It was impossible to render too much praise to the care and exactitude exhibited by the Author in his operations, and the remarks that followed referred wholly to questions of theory or of rightful interpretation.

The Author said (chap. v.) that according to theory the interior pressure of flowing water in permanent motion was less than that in the case of still water, and that it diminished with the velocity. Consequently, if a body of still water were connected, by a tube of small aperture, with a running stream, the level of the latter would be higher than that of the still water.

---

[1] "Recent Hydraulic Experiments."

[2] "Annales des Ponts et Chaussées," Série 6, Tome 4, 1882, p. 43.

Mr. Flamant. He had proved this by experiment, but the difference of level shown had been very small (about 0·07 foot). Here, Mr. Flamant thought, was an error of interpretation. The tube in communication with the running stream would project from the bank. It would in consequence oblige the particles of water impinging against it to take a curved trajectory instead of preserving their rectilinear direction. In the path of this trajectory, of which the concavity would be turned towards the tube, was developed a centrifugal force tending to throw the particles of water from the tube. In this way a kind of suction was set up in the tube, which would diminish the pressure, and consequently lessen the height of the water in the adjacent reservoir, making the latter less than that of the stream, which indicated the real pressure, except in the immediate neighbourhood of the tube. This effect would be the more marked the greater were the centrifugal force, *i.e.*, the velocity of the stream. The Author had been able to show that the difference in the two levels increased with the speed of stream, but he would not have observed any difference at all if the tube had been placed close to the slope of the stream's bank without projecting into the water.

The same would apply, Mr. Flamant thought, to the Author's statement in chapter viii. that the surface of the water should be convex transversally. The Author endeavoured, without success, to measure this convexity. Nevertheless, as the velocity of the water at the banks was nearly nil, the level should be, according to his theory, sensibly that of still water, and, logically, he should have been able to find the same difference between the level at the banks and in mid-stream as between the latter and the water in the reservoir. But between the water near the banks and in mid-stream there existed free communication, without the intervention of any tubes to interfere with the conditions of flow, so that the pressure was the same at equal depths, *i.e.*, the surface was necessarily horizontal. The instance of convexity of surface noticed by Baumgarten in the case of the Garonne did not correspond to a permanent condition, but to a period of rise in the stream, at the beginning of which water would mount more quickly at the middle than near the banks.

Lastly, in chapter xvii., the Author stated that he had noticed near the banks, and on the surface, a persistent current from the bank towards the middle, which current was greatest inshore, and rapidly diminished as it approached the centre. It seemed to Mr. Flamant that this current towards the middle could only be an illusion. When there was placed quite close to the bank a

Mr. Flamant. float, whose dimensions could not be neglected in respect of its distance from the bank, the fluid veins which impinged against it differed in force sufficiently to make the float turn on its axis, and drive it from the shore. This effect would diminish in proportion as the distance from the bank being greater the velocities of the fluid veins became more assimilated. Moreover, a current implied a displacement of liquid; it would therefore be necessary that the water flowing from the bank towards the middle should be replaced by the product of a similar current from the centre towards the bank. The Author thought that there really did exist such a current, for he had noticed that loaded rods placed near the bank were less acted upon than surface-floats, and that they preserved a movement sensibly parallel to the bank. This might be explained by the fact that loaded rods were endowed with a motion comparable to the mean velocity on a vertical, and that all things being equal, this mean velocity varied less than local velocities, and especially than the surface velocity. The Author had himself observed that the curve of mean velocities was often more flat than the transverse curves.

These objections referred only to secondary points; they in nowise vitiated the observations themselves, which had been made with a care and good faith which must be generally acknowledged, neither did they affect the main conclusions arrived at by the Author concerning the discussion of the formulas of mean velocity, and which were amply justified by the observations.

Mr. Gordon. Mr. Robert Gordon remarked that the hydraulic researches published by the Author of the Paper at Roorkee, in 1881, formed a most important connecting link between the previous experiments of Messrs. Darcy and Bazin, and others, on small regular channels with uniform flow, and the more numerous experiments on large natural channels with varying flow, of which the elaborate investigations inaugurated on the Mississippi by Messrs. Humphreys and Abbot formed the type, and, perhaps, the most important example. Neither in his aims nor in his results did the Author strive to raise any new problems; but throughout his inquiries, as narrated in his Report, he restricted himself to ascertaining what light the careful and accurate experiments conducted by himself, and involving an immense amount of conscientious labour of the highest class, could give to the problems now perplexing hydraulicians, and to placing on record for students of the science the elaborate and clearly arranged data gathered in his inquiries. The Ganges canal, at the part where the experiments were conducted, reached the magnitude of a river, both in its cross-section

Mr. Gordon. of nearly 200 feet wide by 12 feet deep, and in its discharge of nearly 7,500 cubic feet of water per second. It verged on the conditions of a river also in its varying and irregular flow; while, from the complete control over some of the principal elements influencing the flow, it retained all the advantages of an experimental channel. No person could peruse the Report without recognising its high value as a permanent contribution to the science. It did not mark out a new era, like the works of Guglielmini or Du Buat, or those of Darcy and Bazin, and Humphreys and Abbot; but it consolidated on a firm basis the results acquired up to the present, enabled the deficiencies of the science to be seen, and might inaugurate a new departure with definite aims and scope. A brief notice of the present condition of the science, as ascertained by the Author, and generally confirmed by other writers, might be presented. Before doing this two exceptions might be taken to Major Cunningham's work, one relating to the matter, the other to the treatment. These were the only ones that occurred to him, and were suggested with diffidence. First, the Ganges canal itself did not offer, in any of the experiments taken, all the conditions for free flow. At the end of each reach of the canal a permanent weir had been constructed to check the velocity of the water; and on the permanent weir temporary weirs were placed, which had the effect of changing the character of the flow throughout the whole reach of the canal above it. These changed conditions were recognised and fully stated by the Author, but the fact remained that in not a single instance was the discharge in the condition of free flow normal to ordinary rivers, and also to experimental canals. How far the analytical results were changed, or if at all, especially in the relations of the velocities on verticals, and in the comparison of the measured discharges with those given by formulas, it was impossible to say.

The second exception as to treatment referred to the use made of the method of least squares in displaying the results of the velocities on the verticals. Obviously, before it was used at all, some law must be assumed to exist and to be known, or else an arbitrary formula must be chosen, and all that the method of least squares could do was to assign the most accurate indices and coefficients to the factors already assumed in the law or formula. There was, therefore, an objection to its use before the law had been ascertained by which the order of the elements changed. The Author assumed that the order of the velocities on each vertical changed like that of a parabola, and determined the particular curve in each case. He also assigned an arbitrary weight, Mr. Gordon.

or value, to the velocities according to their distances from the surface, when taken by the double-float, giving the lower velocities a very much inferior value to the upper ones (chap. xi., p. 4). Theoretically he was correct; but his practice appeared exaggerated, and not sufficiently supported by evidence. As the vertical velocity-curve was the most important of all the analytical results yet obtained, it was desirable to preserve it as free from alteration as possible. Probably the future science of hydraulics would take its principal direction from the teachings of this curve.

That the science needed a new direction was evident from the Author's words, where he summed up the results of his comparison of the measured and calculated discharges of the canal. He tested all the best known formulas in ordinary use, and said:—"The general result of trial of these formulas, which are all empirical, shows that at present increased approximation can only be obtained by increased complexity; there is no guide as to the form of such improved approximations, whilst the labour of tentative research is excessive. Until some guide is obtained from a rational theory, it seems to the Author hopeless to attempt further improvement." This statement was but the echo of almost every writer of authority on the subject within the last few lustres. It had been the one leading object of almost every investigator for a long time to, in the first place, improve and facilitate the mode of research, and the means for calculation for practical engineers; but the further the inquiry was pursued, and the more accurate and elaborate the data, the wider were the discrepancies between practice and theory. Indeed, hydraulics, as it existed, was a science without a guiding theory, as the old theories had been entirely discarded by writers of judgment and experience. But there seemed a diffidence, or a reluctance, or a shrinking from the responsibility of originating a new theory, or even of examining tentative theories on the part of the investigators, to whom practical men looked for guidance in developing the resources of the science to the urgent requirements of modern life. The experienced practician by tact and judgment might overcome difficulties in the field, often at an immense expenditure of time and money, but he could not transmit his personal acquirements, nor enable schoolmen and professors to embody in their text-books and lectures the rationale of his results, unless they were prepared with a well-grounded theory to fit them in to the series of facts constituting the science. So far from throwing new light on old problems, each new investigator, from lack of a rational theory, only rendered them more obscure.

Mr. Gordon. That this was no exaggerated view was shown from Major Cunningham's other results. He investigated the amount and value of the surface-slope of the canal. That this slope was one of the most important elements in calculating the discharge of a stream was well known, yet this was what the Author said of it:— "The general conclusion from over five hundred cases was, that surface-slope measurement is so delicate a matter that the results are of doubtful use." Mr. Gordon could not agree in this conclusion as a general one, though it seemed applicable to the Ganges canal, solely on account of the weirs at the lower end of each reach. From experiments made on the Irrawaddy, now extending over several years, he believed that it was possible, by increasing the number of points and spreading them over a considerable space in order to make continuous or daily observations, to ascertain the general slope of a river or canal with sufficient accuracy to permit it to enter into a formula as a factor. But a very few observations, neither simultaneous nor extended, such indeed as had been too often used in some published results, were most misleading, and, taken with the utter absence of theoretical guidance to indicate the degree in which they should enter into the formula of calculation, landed the user of them in a dilemma of bewilderment.

The Author did not leave the discussion on the practical methods of measuring the discharge of an open channel in a satisfactory condition. For small regular channels there was not much difficulty; but for natural channels, and even for the Ganges canal, the final solution as to the best instrument of measurement had not been obtained. He favoured the double-float; in this agreeing with Messrs. Humphreys and Abbot and himself. But the Author did not treat the results with sufficient respect in his use of the method of least squares. He also approved of the rod, which apparently answered well in experiments like his own, and those of Mr. Francis at Lowell, in regular-shaped channels, not of great depth. But in moderate-sized channels of irregular shape, it had been found by the experimenters in the Rhine in Holland, that the rod was incorrect and unsuitable. They and other continental investigators appeared now to have entirely adopted the Woltmann meter in one or other of its various forms. The Author, however, seemed unequivocally to condemn these, so far as his experience extended, and quoted General Abbot with approval, who held the same view still more strongly. Mr. Gordon had for several years carried on extensive experiments on the Irrawaddy with double-floats, and had endeavoured during the last flood-season (1882) to

Mr. Gordon. test these with three of Deacon's electric current-meters. The Indian Government gun-boat "Irrawaddy," with a large staff, was at his disposal, and most elaborate tests had been made. Two of the meters had been sent to be re-tested at Torquay by Mr. Froude, who originally ascertained their coefficients at the Admiralty experimental tank, and the result was not yet known. But from a careful examination of the results of the experiments, which were conducted in depths of from 12 to 100 feet, and up to velocities of 8 feet per second, it was feared that the instruments changed their rate in the silt-laden water, which also carried an immense quantity of fibrous vegetable matter in a fine state, the lowering wire and meter often coming up covered and clogged with this.

While, therefore, he believed that hydraulicians were deeply indebted to Major Cunningham for his valuable labours, and for placing the true state of the science so conspicuously forward, it was much to be regretted that it was not found possible to bring within the scope of the work some discussion or indication of the rational theory, by which alone the present confusion could be overcome, and real progress ensured. The Author had already shown in separate Papers that when he took up this branch of the science he would do much to elucidate it, and when the clue was found it was hoped that the present experiments would find their true interpretation, with those of others, in building up a sound structure of theory.

Mr. Hagen. Mr. G. HAGEN, of Berlin, held that the laws hitherto discovered on the motion of water in rivers were of no value practically. The subject was too complicated, and very difficult, and the observations had not been sufficiently exact. He had already given his views respecting the Roorkee hydraulic experiments in the "Zeitschrift für Bauwesen."[1]

Mr. Hollings. Mr. J. S. HOLLINGS remarked that, were it possible to ascertain with accuracy the mean evaporation of the whole globe, it would most likely be found to be exactly equal to the rainfall (for rain was but condensed vapour); otherwise the sea would be either advancing or receding on all shores alike, for the ultimate goal of all water not evaporated was the sea. Living as he did in Montserrat, a small island in the tropics, he had ample opportunity of observing how very little rain fell on the sea, compared with the land. He felt sure, if correct measurements of evaporation from the sea could be obtained, they would show a large excess over the

---

[1] Band xxxi., p. 403. Berlin, 1881.

Mr. Hollings. rainfall on its surface; whilst on the other hand, the rainfall in a mountainous country would be in excess of the evaporation. So many causes, however, tended to modify evaporation, that it was almost impossible to keep such a gauge correctly. The temperature in the sun in the West Indies rose to about 150° Fahrenheit on hot days, and was rarely lower than 78° in the shade, calling the shade the coolest place available for a thermometer. This latter was also the mean temperature of the sea, whilst 80° was that of the air over the land in the coolest place. On some days with little wind, the wet bulb of the hygrometer in the shade was only 1° less than the dry, whilst on other days, with the same sun-heat, 10°, and occasionally 12°, difference were registered, the only altered condition being the movement of the air. The evaporation from equal surfaces of water on sea and land would most likely also differ materially under the same conditions of sun-heat and air-movement, for the vast reserve of unheated water in the sea would continually retard evaporation from the surface water, whilst the comparatively thin stratum exposed to the sun in lakes, rivers, canals, reservoirs or pools, would soon get heated through and rapidly evaporate. It was therefore almost impossible to obtain sufficiently reliable results from any number of evaporation gauges on land, or floating in terraqueous suspension, to form a basis for calculating the mean evaporation of the globe. He believed an evaporation-gauge had been kept at Barbados for several years, and that the average evaporation was about 3 inches per month, which agreed with the Author's $\frac{1}{10}$ inch per day. The maximum evaporation was, he thought, under 4 inches in any month; whilst the average rainfall was about 54 inches per annum. The hygrometer readings in Montserrat agreed pretty closely with those at Barbados, whilst the rainfall at sea-level was about 46 inches; at 250 feet above sea-level, 56 inches, and at 1,200 feet above, 84 inches. Thus, wooded mountains of only a little over 1,000 feet elevation, led to nearly double the rainfall, whilst at the same time they probably lessened evaporation by more than half; for at the higher elevation he had never seen a difference of more than 5° Fahrenheit between the wet and dry bulb, and rarely more than 2°. All this tended to prove that, until a series of observations embracing different grades of level of land, and varying depths of sea, both for evaporation and rainfall, were collected, the question of whether the one was in excess of the other could not be answered.

Mr. Leslie. Mr. JAMES LESLIE observed that the experiments showed a smaller velocity at the surface of the water than at some depth.

Mr. Leslie.

Might not this arise from the floats being slightly above the surface of the water, and so being impeded by the air, if calm, or still more if there was some wind in a direction opposite to that of the water? The average velocity seemed to have been taken with great care from many points at different depths, and at different horizontal distances, and if the sectional areas were given, would afford accurate data for calculating the discharges. The surface fall in any ascertained length might have been given with advantage, and either two or more cross-sections in that length, or the sectional areas and rubbing surfaces, so as to afford means of ascertaining the mean hydraulic depth, and thereby the proper coefficient for any formula adopted, for arriving at an approximation to the velocity and discharge of any river, without having to go through the troublesome operation of finding the average velocity. He thought the alleged difficulty in ascertaining the surface-level, owing to the undulations, might have been obviated by having a vertical tube or cylinder, with a small orifice at the bottom like a marine barometer or a tide gauge.

The experiments on the evaporation from the surface of water, showing it to be only $\frac{1}{10}$ inch per day, were important, as correcting a widely entertained notion that in Great Britain the evaporation from the surface of water was much greater than that stated, and even considerably more than the loss from agricultural or pasture land.

He had found, from many observations, that in dry summer weather, evaporation from a surface of water in Scotland was only $\frac{1}{12}$ inch per day, and throughout the year it was considerably less than the loss from the surface of land having vegetation on it, showing that it was a mistake to deduct the surface of a lake or reservoir from that of a gathering ground.

As the Indian observations on the evaporation from a surface of water were generally made during dry weather, those observations during wet weather being mostly rejected, it might be assumed that the average evaporation for the whole year would be much less than $\frac{1}{10}$ inch per day.

The evaporation was from the surface of water in a vessel about 1 foot in diameter, and from two zinc tubs about 2 feet in diameter, filled with earth and growing grass, the one outlet being at the surface representing undrained land, and the other at the bottom representing drained land.

Mr. Leslie.

Observations made at Fernielaw, Colinton, on the North Slope of the Pentlands and 500 feet above Sea-level.

Date.	Rain Gauge.	Loss from Surface of Water.	Loss by Deep Drainage.	Loss by Surface Drainage.
	Inches.	Inches.	Inches.	Inches.
1871	35·2	19·4	16·4	20·4
1872	49·1	13·0	19·4	18·2
1873	34·4	12·8	15·5	14·7
1874	32·0	18·0	18·7	18·9
1875	34·1	14·2	17·6	17·0
1876	42·9	15·4	19·7	19·3
1877	45·7	15·2	17·1	16·3
1878	34·4	18·9	20·0	22·2
1879	39·1	12·7	15·9	15·5
1880	34·4	14·5	18·7	18·3
Average	38·13	15·41	17·90	18·08

Rainfall, and evaporation from the surface of Water in a Tub 6 feet in diameter, and 3 feet deep at Glencorse Filters on the South Pentlands, 650 feet above Sea-level. The Level of the Surface of the Water when set at Zero is 1 foot below the top, thus making the depth only 2 feet.

Date.	Rain.	Evaporation.	Date.	Rain.	Evaporation.
	Inches.	Inches.		Inches.	Inches.
1857	34·90	12·30	1869	34·15	14·15
1858	28·75	10·90	1870	27·20	11·90
1859	35·05	12·60	1871	34·45	10·30
1860	30·00	9·50	1872	52·20	9·25
1861	38·50	10·00	1873	36·30	10·15
1862	42·80	10·25	1874	37·75	11·60
1863	38·90	12·45	1875	37·90	11·95
1864	35·75	12·15	1876	45·35	12·11
1865	35·60	9·50	1877	54·30	11·05
1866	37·50	9·10	1878	38·40	13·95
1867	37·75	10·45	1879	46·75	11·65
1868	46·00	14·70	1880	45·00	12·10
			Average	38·80	11·42

Mr. McMath.

Mr. R. E. McMath, of St. Louis, Mo., U.S.A., observed that the theory upon which slope-formulas were based required uniformity of cross-section as an indispensable condition. The longitudinal section, Plate 1, "Roorkee Hydraulic Experiments," showed that the immediate vicinity of the experimental sites did not satisfy this condition. Therefore verification of slope-formulas could hardly be expected from the experiments, and it would not be fair, upon their evidence, to press the conclusion that empirical formulas were of "uncertain applicability." Limited in application they certainly were.

Likewise the underlying theory of discharge-tables did not allow of change in the conditions of out-flow from the reach. The experimental sites were subject to the influence of the "State of control," and consequently observations should alone be compared, which had been made under similar conditions of obstruction in distributaries and at the tail of the reach. Any change in these conditions required a new table rather than complication by "double entry."

As a first impression, it might seem that the tendency of the work was to discredit formulas and discharge-tables. A truer interpretation of its teaching was that both must be used within rigid limits, and not loosely as had been the practice. Both methods were useful when properly applied, and were indispensable to the engineer, the one when designing, the other for continuous-discharge determination. But this necessity attached to the essentials, and not to forms.

The Author said in regard to formulas, "Until some guide is obtained from a rational theory, it seems hopeless to attempt further improvement." An important step towards a rational theory and practice would be effected when recognition was taken of the wide difference between an artificial channel of unvarying section and uniform bed-slope, and the natural stream in which both varied continually. The use of a formula containing slope and hydraulic mean depth was strictly limited to the former. Otherwise $S = \frac{\text{Fall}}{\text{Distance}}$, and $R = \frac{\text{Sectional Area}}{\text{Wet Border}}$, were not properly associated together, S being for a distance, and R for a single point in that distance. To be logical, $\frac{\text{Cubical Contents of Reach}}{\text{Wetted Surface}} = \rho$ should be taken instead of R. In such case the particular section chosen for a site should be that in which $R = \rho$, the latter being determined for the full slope-length. This involved another condition. The local values of R must either increase or diminish

Mr. McMath. throughout the slope-length, in order that ρ might be a hydraulic mean, as distinguished from mere arithmetical mean.

These conditions had not been observed, even by the originators of slope-formulas, and yet, he thought, need only be stated to be accepted as controlling.

In a natural stream the unequal distribution of fall suggested that surface gradient, local slope, measured the retarding rather than the accelerating force, answering to the force expended in overcoming resistances, rather than to that which produced motion. Momentum and facility for transmission of pressure had to do with motion in open as well as in inclosed channels, for the current often continued when slope was negative. A rational theory recognising these facts would have regard to head and not to its accidental distribution.

The possibility of useful discharge-tables—and there were such—was so closely connected with the better theory, that to show the conditions of the regular law of discharge implied in a table was, so far as it went, the development of the theory.

1. For a discharge-table, it was essential that the preliminary observations should be made at the site for which the table was to be prepared; or that simultaneous gauge-observations should be made and referred to a common datum, so as to refer each observed discharge to the proper local height.

2. The cross-section should be a regular figure, at least above the level of no-flow, preferably a rectangle.

3. There should be a considerable wet area remaining when the water was drawn down to the lowest possible level. For area controlled the increments of velocity, and determined a discharge-curve, which, if not simpler, was at least flatter, and therefore determinable with less percentage of error than when area became small. This condition located the discharge section in a "marked hollow in the bed-slope," and required it to be in the obstructed sub-reach.

4. In the obstructed sub-reach, the crest of the fall defined the level of no-flow. In a river, the crest of the shoal defined that level in the reach above. This level was different from the plane of low water. It fixed definitely the origin of the discharge-curve.

5. The conditions of discharge from the reach must be invariable. The utility of discharge-curves rested upon their affinity to a weir-formula; or that the discharge over a given weir, whether free or submerged, was for a given head a definite quantity. If the opening at the weir were changed, in any way, the discharge would

be affected. The opening of side-channels (distributaries) might Mr. McMath. be the equivalent of lowering the crest, as well as of increasing the area of discharge.

6. The conditions of approach to the reach should be such, that currents should follow the same general direction at high and low stages, and that no eddy should at any time exist at the discharge-section.

7. In a river it was important to select a site remote from a tributary, if possible, but below rather than above.

By observing the above conditions the results would be reliable if the local law of discharge was well determined.

For such determination a series of observations would be necessary, covering a range of stage as wide as practicable. The conditions stated were intended to make the discharge conform to an equation of the second degree. Since adopting the Author's notation, $D = AV$, an equation of the second degree implied that the varying values of A and V should each follow the law of a straight line. In a rectangular section this was secured for A.

$$A = b\,h.$$

Also if the section was irregular below the level of no-flow and rectangular above, for the area below no-flow level might be represented by a constant, a, and

$$A = a + E\,b.$$

E was the elevation of surface above the level of no-flow.[1]

For the velocity-law recourse must be had to the discharge at the fall, and to the conditions of motion in the obstructed sub-reach.

The head of the sub-reach was defined by the intersection of the line of no-flow with the "thalweg" profile. Taking the elevation of the surface at the head of the sub-reach above the crest of the fall (the weir, not water-crest), E was obtained, the head which controlled motion in the reach. A certain part of E was consumed in overcoming resistances; the remainder, $E_1 - E_2 = E_3$, was found at the fall where the discharge D, definite for a given value of E_3, passed through a section of fixed dimensions A, with a mean

---

[1] In a paper read before the American Society of Civil Engineers, No. ccxxxix., vol. xi., Transactions, 1882, this element, new to hydraulics, was named "ruling depth," and was represented by Δ, but as that symbol was appropriated, Mr. McMath therefore now substituted E, an abbreviation for "elevation above weir-crest."

Mr. McMath. velocity V, due to the fall E_3. At no other point in the sub-reach was there a velocity approximating that due to the fall, local or aggregate, being clearly too great for the local Δ E, and too small for the aggregate E. At any section with an area A, the mean velocity, V, would be the quotient of $\frac{D}{A} = V$, and $\frac{V_1}{V} = \frac{A}{A_1}$. This relation of velocities at the section in the reach and at the fall held at all stages; consequently, if there was a law at the fall, there must be at every section in the reach a direct relation between E and V.

If consideration was given to the increments of V at any section for definite increments, e, of E, evidently $\frac{A}{A + e\,b}$ and $\frac{A + e\,b}{A + 2\,e\,b}$ would differ but slightly, and by a nearly constant difference, if the initial value of A was large. This was provided by the third condition, therefore the actual values of V would be closely approximated by the equation of a straight line,

$$V = c\,E + d.$$

Combining with $A = a + b\,E$, then

$$D = A\,v = c\,b\,E^2 + E\,(a\,c + b\,d) + a\,d.$$

For the coefficient c, and constant d, the measured discharges must be depended upon.

Within the ordinary range of measured discharges it would be found that, the better the observations, the more closely would the mean velocities approach a straight line. But if accuracy was reached in the first decimal place, the fact that the law of mean velocity was of the second degree became apparent. The curve was very near a hyperbola,

$$V = \left(\frac{A^2}{B^2}E^2 + A^2\right)^{\frac{1}{2}} \pm A,$$

A being the transverse axis, and an apparent function of $\sqrt{2\,g\,E}$, B being the conjugate axis and an apparent function of mean depth, or hydraulic mean depth.

Since curves were by observation concave or convex to the axis of E, according as mean depth was less or greater than an undetermined limit, it was suggested, that when the hydraulic mean depth of discharge section was equal to $\rho = \frac{\text{Cubic contents of reach}}{\text{Wetted surface}}$, Mr. McMath.

the velocity law was a straight line.[1] This furnished another condition of choice for a permanent discharge site.

8. The local value of R should approximate that of ρ for the obstructed sub-reach.

According to the view taken, the section at the crest of the fall was the limiting section for the sub-reach; but this was true only as it was the section of least area. For the head, E, in the reach was that required to force the discharge through the smallest area. The relations shown would be destroyed, if the site chosen was above a part of the reach so narrow that the area at high water might be less than at the crest of the fall. The limiting section must be least in area and depth. The argument had followed the case of a dam of free overflow, in order that the definite value and influence of E might be unmistakable. The relations would be no less useful if the dam was submerged, or if it was a natural shoal.

Hints of the more rational theory had appeared in the foregoing discussion. It might therefore be appropriate to add, in closing, that the better theory could only be reached and tested by hydraulic experiments, whose results should be cleared of the effect of "unsteady motion." This could not be done by any method of direct velocity measurement; but could be by making an absolute measure of the quantity discharged in a given time. Mr. J. B. Francis, in his Lowell experiments, obtained mean velocities accurate to the fourth decimal place, as was evidenced by systematic residuals when compared with approximate computed curves, good to the second decimal place. With such data to work from, the way to a rational hydraulic theory would not be long.

Mr. Neville.

Mr. JOHN NEVILLE thought the measurements to obtain the average velocities and depths (pp. 5 and 6) should have been extended for longitudinal as well as for cross-sections. The width should have been divided into several sub-runs or sub-channels, moving side by side according to circumstances, but not less than three. Such measurements would give a simple and sure operation for finding the discharges from top to bottom within the widths of each sub-run; and between the banks, by adding the sub-discharges together (similar to the use of offsets to a chain line in surveying), which would obviate the use of formulas (p. 25), by suitable admeasurements.

The means of determining the bed-, side-, and mean velocities

---

[1] If this suggestion was not strictly correct, it was at least an approximation, and of value as a practical guide in selecting a site.

Mr. Neville. was not very clear. The Author said (p. 23), "The decrease of forward velocity is so rapid close to the banks as to make it clear that the forward velocity must be very small;" "it does not admit of direct measurement." And again, "In a float-course only 7½ inches from a straight vertical bank one hundred surface-floats were run before three (3) were obtained in fair course over a 12½-feet run." Mr. Neville had himself often found a backward surface-flow near the banks of rivers. It was stated (p. 8) "that the range of velocities, deduced from a number of similar floats run in rapid succession over nearly the same float course, was commonly 20 per cent. of the mean. In some of Harlacher's experiments a current-meter was fitted with electrical connections so as to record every revolution; the variations amounted to from 20 per cent. in surface-velocities to 50 per cent. in bed-velocities in a few seconds." He had often observed that the flowing section of a river did not always correspond with the section of the channel taken to the water-level. Yet an average forward discharge took place, notwithstanding many changes of direction in the motions. Practically, taking the mean velocity as v, and the maximum surface-velocity as V in feet, the formula deduced from Du Buat's experiments by Prony, viz., $v = \frac{7 \cdot 783 + V}{10 \cdot 345 + V} \cdot V$, gave more consistent practical results for all channels in the run of professional work than the more complex formulas of later writers on the subject.

The falls, of about 8 feet, at the lower ends of the reaches on which the observations were made, must have affected the results, and depressed the surface maximum velocities down towards the level of the crest of the overfall or sill of sluices. Great disturbance must also have existed below the foot of an upper, and head of a lower, reach. The depression of the maximum surface-velocity from wind and atmospheric causes was only occasional and partial, although the earth and the atmosphere formed a sort of compound tube for the water section to flow in. Notwithstanding that the vertical velocity-curves were "all very flat" (p. 14), so flat that "any geometric curve could be fitted very close to them" (p. 15). The statement, therefore, that the "last result is of great scientific importance," viz., that "the vertical curve is nearly a common parabola, the error of whose computed parameter is often very large," was scarcely justified. Also the statement that "no confidence can be placed in values of the parameter not formed by the method of least squares;" concluding with: "After many attempts to construct a new formula, the conclusion is drawn that the data are too uncertain to admit of it!" Page 16 would seem to show that these experiments were productive of little useful result.

Mr. Neville.

That the transverse surface-curves (pp. 12 and 13) must have their convexity or concavity always small, dependent on the depths below and corresponding surface-velocities, has been well known for a long time. The great number of Roorkee experiments added nothing new to the scientific knowledge of these curves.

Observed levels of the water-service at and above the falls, in the stretches, would have given useful data for the construction of the backwater curves of much more practical and scientific value than any vertical-velocity or transverse surface-curves.

The surface-slope or longitudinal inclination of rivers was apt to lead to a great deal of misapplication of hydraulic formulas. It was so flat that, unless at rapids, a considerable change was not always apparent. The range in these observations is from 1½ inch to 30 inches per mile, or from 0·0284 to 0·568 inch, nearly, in a float-run of 100 feet. The levels themselves varied 0·07 foot, or 0·84 inch in the tranverse section (p. 6). "From twelve trials of slope-lengths of 2,000 and 4,000 feet, symmetrically situate about the same site," the slopes differed 25 per cent. (p. 10), and slopes at opposite banks differed 50 per cent. (p. 11). To find the value of the slope and for what run to take it so that it should correspond with the hydraulic inclination was practically next to impossible. Mr. Neville often found wind to affect the bank water-levels; and on the callow lands along the Shannon between Athlone and Portumna he had observed sometimes a difference up to 1½ inch at different times on each bank. There is an undulating wave-surface and ripple on rivers of any depth which affects the forward motion, causing it to vary from time to time in the same transverse section, and even simultaneously in different ones, the slopes varying very considerably, although the difference in level may be small. Slopes of 1½ inch and 6 inches in a mile gave only about 0·3 inch and 1·2 inch in over 1,000 feet, yet the small difference, 0·9 inch, in this distance involved the doubling of the velocity. He remembered a law-suit in which one engineer, calculating the flow in a mill head-race from the surface-slope, and another from floats, differed as 3 to 1. Not only did the surface-slopes vary in the same river, but so also do the depths, including the hydraulic quantities *r* and *s* and the area of the section in motion.

Mr. Neville had considered elsewhere[1] the various formulas

---

[1] "Hydraulic Coefficients, Tables, and Formulæ." Second edition, pp. 182 to 226. Also Third Edition, pp. 188 to 251.

Mr. Neville. enunciated by their Authors from Du Buat to Weisbach and since; most of which were applicable to pipes and rivers, properly understood, r and s being factors in all. In pipes there was no limit to the inclination from flat to vertical; for canals and rivers it never exceeded a few feet per mile. The experiments from which those formulas were deduced were select and varied, ranging from pipe-diameter of ½ an inch and less to 18 inches, to open channels, large canals and rivers. There were differences, and would continue to be, between a formula grasping so great a range, in size and inclination and particular experiments; but this was of no practical importance. Very little had been done since to improve this practical branch of hydrodynamics. A formula should be framed meeting any particular group of hydraulic experiments working within practical limits; it must be in part empirical, not too complex, and capable of application to vertical short pipes as well as flat long rivers. Du Buat conceived, and first constructed, a formula to meet these conditions, using one hundred and twenty-five experiments, carefully corrected for the entrance-velocity on both closed and open channels, pipes, and rivers. Nothing better had been done since.

The results to science of his hydraulic experiments was stated by the Author to be "that at present increased approximation can only be obtained by increased complexity; there is no guide as to the form of such improved approximations, whilst the labour of tentative research is excessive. Until some guide is obtained from a rational theory, it seems to the Author hopeless to attempt further improvement." "A guide," and "rational theory" were given by Du Buat long ago. The surface-slopes of open channels of any good size were so small in fall, and varied at the same time so much in ratio, that they would never be used by competent engineers or by experimenters to determine or test the observed mean velocity by the form of equation $v = m\sqrt{r\,s}$, or any other; though they might from the equation $s = \frac{v^2}{r\,m^2}$ determine the slope where experiments would fail. Besides, the surface-slope in most cases was not the hydraulic inclination, nor was the average slope in 2,000 or 4,000 feet, more or less, that also found at intermediate points or at the float-runs from 12½ to 100 feet long. It rested with the experimenter to determine accurately the values of s, r and m before giving an opinion on the value of any formulas containing these elements.

Lieut.-General Rundall. Lieut.-General F. H. **Rundall**, R.E., C.S.I., had already reviewed the subject of the Paper under discussion in "The Royal Engineers'

Lieut.-General Rundall.

Journal."[1] Two points of special interest connected with the experiments might be referred to. One point, theoretical, was the convexity of surface in rivers at certain stages of their floods; the other point, practical, was the effect of silt in suspension on the flow, the proportionate quantities carried at different velocities, and the quantity or proportion deposited on a diminution of velocity. This last was of great importance in the case of canals in India, where silt clearances had often to be annually made, entailing one of the heaviest items of "annual repairs." But "silt" was understood differently in Northern and Southern India. In the Ganges Canal it consisted mainly of material derived from erosion of the bed and sides of the canal, owing to the comparatively high velocity maintained in the upper reaches. In Southern India silt was understood as the solids held in suspension in the river, and deposited at every change in the velocity. That in the Ganges Canal was not fertilizing, except at periods of the year when the River Ganges happened to be in flood. In the Delta rivers of Southern India the silt was invaluable, and the more of it that could be conveyed to the fields the better. The problem with engineers was how to take into the canals as much silt as possible, and to allow as little of it to be deposited in the canal consistent with maintaining as low a velocity as was advisable for navigation purposes. He noticed that the Author stated that the result of his experiments in this respect was disappointing, and that the Ganges Canal was not a suitable work for testing the relations of silt to velocity.

Mr. Stone.

Mr. T. W. STONE considered that the results given in the Paper did not admit of dispute, as they agreed generally with received opinion on such questions. There were, however, a few points on which he desired to remark, solely with the desire of eliciting further information. The primary objects of the experiments were, he apprehended—

1. The discovery of a good method of discharge-measurement.
2. Testing the applicability of known mean velocity formulas.
3. The discovery of a good approximation to mean velocity.

The two first objects had been attained. In regard to the third, he thought that although a practical result had been achieved in regard to existing canals, the like could not be said in reference to projected canals, or the investigation of floods in rivers, beyond

---

[1] Vol. xii., p. 65, March 1882; also p. 92, April 1882.

Mr. Stone. the statement that Kutter's formula for variable coefficients, applied to the old expression C $\sqrt{RS}$, would give results seldom exceeding 7½ per cent., and that such results fall far short of that given by direct discharge-measurement. In the concluding paragraph of Section XXI., "Discharge-Verification," a 5 per cent. difference was called a close approximation, and it was shown in Section XX., "Mean Velocity," that Kutter's formula would give results seldom exceeding 7½ per cent.; it followed, therefore, that the Author's experiments tended to establish Kutter's formula as a close approximation for mean velocity in canals and channels 200 feet wide and under. Now Kutter's formula in effect proposed the use of the old formula $v = C\sqrt{RS}$ with variable coefficients, and to obtain such coefficients the two variable factors S and R were used. The formula agreed nearly as well as the discharge-verifications, yet the Author said, Section XIV., "It was found that the direct measurement of any velocity was far more likely to give an approximation to this mean velocity than any expression yet known not involving velocities." And again: "It seems, then, that at present the direct measurement of velocity, such as the central mean or central surface, is more likely to give a near value of the mean velocity than any formula involving surface-slope." It should be constantly borne in mind that with factors R and S Kutter's formula for variable coefficients agreed to 7½ per cent., while tests for discharge-verification varied, on the Author's showing, 5 per cent. and more. A wide range of experiments would be necessary to determine what the central surface or central mean velocity was likely to be in any proposed channel without use of formulas; and such experiments must extend to channels of not only different sizes, but of all various inclinations and descriptions. It was necessary, therefore, that some formula should be used for the design of projected channels, the investigation of floods in rivers, &c., &c., and Mr. Stone maintained that a set of variable coefficients of mean velocity of discharge should be used in accordance with the circumstances of each special case, and the nearest similar recorded observation that could be obtained. This should be so with all hydraulic calculations. There was no necessity to alter the old theoretical formulas; the coefficient expressions might vary as experience became enlarged. Such expressions as $v = C\sqrt{RS}$, $v = C\sqrt{R(f2)}$, used with a variable coefficient determined on the lines proposed by Kutter, would give fairly approximate results. The importance of using variable coefficients was pointed out in Mr. Stone's recent work, "Hydraulic Formulæ," and was, he thought, always followed by engineers

Mr. Stone. experienced in hydraulics. The Author's experiments had been valuable in showing that such a course led to fairly approximate results. The channels constructed by Mr. Stone in Australia were calculated from the formula $C \sqrt{R\ (2 f)} = v$, and C was varied from 35 to 75, to give mean velocity in feet per minute, according to the nature of the proposed channel. The channels varied in section from 2 feet square to 6 feet at bottom 6 feet deep, and slopes of 1 to 1 and 1½ to 1, and some of the channels had a circular lining; the inclinations varied from 2 feet to 10 feet per mile, and the formulas gave very fair results. To establish the statement of the Author, above quoted, further confirmation would seem desirable. He thought that the assertion in Section III., "As the canal-water is often full of silt, this shows that the water is in pretty rapid motion close to the actual bed, and disproves the idea sometimes advanced that an obstruction across a channel causes a still-water pool above it roughly flush with its crest," should be accepted with caution. Whilst Government Engineer in Australia, Mr. Stone built a series of obstructions from 8 to 11 feet high across a channel for the same purpose as the falls on the Ganges Canal were built up, viz., to reduce the velocity. The channel in question was about 30 feet wide at the top, and irregular in section, on account of the previous velocity having been too great for the bed and banks. The large amount of silt which accumulated in the reaches above the obstructions in this case went to show that the Author's deduction was not invariably correct; possibly the frequent exercise of the control spoken of at the falls for working the canal might have tended to produce scour in the bed, and so prevent silting. At Section VI. the Author first referred to the fact that both parallel motion and steady motion did not exist even approximately in flowing water, and assumed in effect that all formulas based on such motions must be incorrect. Finally, however, he admitted that there was an average steady motion. Though of opinion that formulas constructed on the theory of steady flow would be fair approximations in connection with variable coefficients, he agreed with the Author that any single velocity-measurement would be clearly an accidental value. At Section VIII., "Surface-convexity," the Author stated that the fair conclusion seemed to be that "the water-surface across is probably level on the average." As a general rule Mr. Stone thought the cross-section of a stream showed a horizontal line for the surface of the water only when a uniform depth and velocity obtained right across, or nearly so. If one part of a channel were much deeper than the rest, and the velocity of the water was

Mr. Stone. much greater in that part than in the other parts, the level of the water became raised there, so that the surface line became uneven. When the stream was confined to only a portion of the channel, the rest being still, or backwater, the irregularity of the surface was greatest. This was exemplified by observations taken during an investigation of the floods in the river Barwon in Australia, in 1880.[1] It had also been observed in some of the large drains in Melbourne, Australia. According to Mr. Culcheth, the velocity of the stream of the Barwon was 5·4 feet per second, and the river 1,400 feet wide. The mid-stream level was convex with regard to one bank, and concave with regard to the other. The Melbourne drains spoken of were about 9 inches wide at the bottom, with sloping sides of 1 in 5 on one side, and 1 in 10 on the other. They sometimes ran from 1 foot to 1 foot 6 inches deep, and had varying inclinations.

Prof. von Wagner. Professor VON WAGNER did not possess the complete work of Major Cunningham, but only the Paper read before the Institution, which treated in separate chapters the principal conclusions, and in some cases dealt with them more fully. It would therefore be simplest to give his opinion in the order of those chapters, and to add in conclusion some remarks related to the subject.

IV. He agreed with this entirely as regarded surface-floats. Large bodies, such as boats, floated, it was said, with greater velocity than the water, but small bodies, such as spheres, or disks, might well be used for the measurement of normal velocity, because, as was proved by all experiments, no sensible acceleration could be detected. Of many experiments on this point, he would only name those on the Rhine at Speyer, in which two carefully-adjusted instruments, a screw Current-Meter and a Darcy Tube both gave a velocity of $v = 2\cdot17$ metres, while nine surface-floats, with a path of 100 metres, gave $v = 2\cdot18$ metres. To get accurately the horizontal curve of surface velocities he selected in the transverse water-line not too many positions (for a float-path of 100 metres, about seven or eight), and then for each position he observed at least ten to twenty floats. He considered this better than to select twenty positions and to observe at each four or five floats. It was very prejudicial that the wind had so much influence on floats, and that good results were only to be obtained when the air was nearly still.

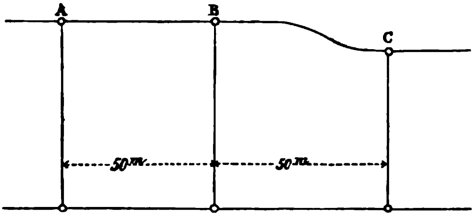
---

[1] W. W. Culcheth, M. Inst. C.E., Paper "Floods on the Barwon," read before the Royal Society of Victoria, December 8th, 1881.

Prof. von Wagner.

Further he thought it important to consider the longitudinal profile of the water-surface (Fig. 8). If, for example, the surface-velocities at a cross-section B were to be ascertained by a number of floats, the path of the floats being about 100 metres (A B = B C = 50 metres), then it might happen that in one-half of the path B, the fall might be greater than in A B. If the surface had such a form, then in his opinion the float-path should be shortened and the number of floats observed should be increased, in order to obtain a mean velocity.

Fig. 8.



As to those floats which reached deeply below the surface (loaded rods or coupled balls), he had hitherto not been convinced that they gave accurate results. His objections to them were stated in his "Hydraulic Experiments on the Weser, Elbe, and Rhine," p. 3.[1] Whenever accurate measurements were required he preferred good current-meters, such as those of Amsler-Laffon, Harlacher, and others. But of floats he only used surface-floats, and these only to check the coefficients of the current-meters, or to obtain approximate values as indicated below.

V. Agreeing with this, he would only add in regard to the measurement of the slope, that, if there was a rippling surface, he had always made a small side basin close to the river, in which the surface was kept still by a floating plank or even by pouring on some oil, and thus the reading was made more definite. He was led to do this by the opinion that the water-level so obtained would be a correct mean between the wave-crest and wave-trough.

He quite agreed with VI., especially with the final words "Taking" to "motion." As to theory in the proper sense of the word (hitherto everything was but empirical formula), it could

---

[1] "Hydrologische Untersuchungen an der Weser, Elbe, dem Rhein und kleineren Flüssen." Braunschweig, 1881.

Prof. von Wagner.

not concern itself with eddies, &c., but must be based on normal conditions. The chief difficulty was the necessary modification by experimental values, coefficients, &c.

His views also coincided with those expressed in VII. Whenever the longitudinal profile of the water-surface had the form of a long curve, then, according to Harlacher's method, the true slope was the tangent to the water-surface at the intersection of the wave-form with the cross-section considered.

He had found as a rule that the longitudinal slopes were the same at both banks, if the river at the site had a normal bed and a straight direction. When there were irregularities, then two levellings, one at each bank, were sufficient; but with large rivers one might be desirable in the middle, *i.e.* in the thread of the stream; its measurement was, however, exceedingly difficult.

VIII. He had also had an opportunity of observing the transverse water-line, and had given the particulars in "Hyd. Untersuch.," p. 42.

X. The statements confirmed what he had found in other rivers:—

(*a.*) That generally the vertical curves were more bent, the smaller the depth of water, and *vice versâ.*

(*b.*) That the mean velocity at a vertical was smaller than that at the half depth.

XI. Differences of opinion still existed as to the position of the parabolic axis. Some placed it vertically, with the vertex of the parabola at the river bed. The majority placed the parabolic axis horizontal and at the point of greatest velocity. It was interesting to him that Major Cunningham placed it horizontally, as he did himself. His experiments, giving a considerable number of carefully measured vertical curves, proved that with a vertical parabolic axis were obtained results altogether useless, because the normal parabola deviated so quickly from the measured curve, that there was no dependence on the position of the vertex at the river-bed. On the contrary the results were satisfactory for the other position of the axis. In his investigation of sixty-four curves of small streams and large rivers he had come to the following conclusions:—

(*a.*) Down to a point at depth M, Fig. 9, (on the average at 0·8 of the whole depth) the curve consisted of a parabola A M, but below M, of a curve deviating from the parabola, the water being retarded by the bed. A similar deviation, though to a smaller extent, was found from A to W, possibly in consequence of the friction of the air. The vertical curve thus consisted of three

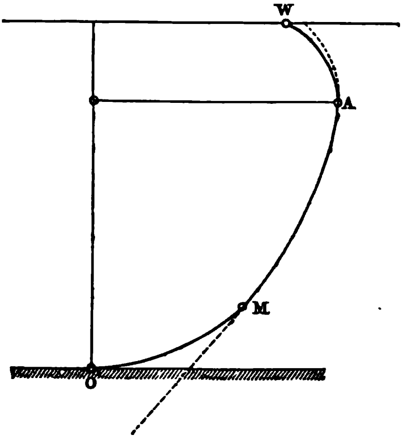
Prof. von Wagner.

parts, of which the middle one, which was parabolic, had the greatest length.

(*b.*) The axes of the parabolas were horizontal (or strictly parallel to the water-surface).

(*c.*) The maximum velocity (or parabolic axis) was at from 0·0 to 0·28 of the whole depth below the surface. In the Mississippi, at 0·3. Even those curves, which were measured in a perfect calm, showed the parabolic axis below the water-surface. More details of this would be found in "Hyd. Untersuch.," pp. 39, 40, and Plate 8, Figs. 86 to 86*c*.

FIG. 9.



XIV. The formulas here given he had compared with the values in a number of different measured curves (Weser, Elbe, Rhine, Danube, &c.), and he had arrived at the conclusion that the second formula,

$$U = \tfrac{1}{2}\left(v_{0\cdot211\,H} + v_{0\cdot789\,H}\right),$$

gave more accurate results than the other. In most cases it agreed perfectly, the differences being never more than $2\tfrac{1}{2}$ per cent.

With regard to the other statements in Section XIV., as to the position of the mean velocity on a vertical, he would refer to p. 37 of his "Hyd. Untersuch." It was there shown how little the position of mean velocity differed in the most different curves. Now if to these results were added those on the Mississippi, as well as that given in Section XIV. of 0·62 or $\tfrac{5}{8}$, the following figures resulted :—

Prof. von Wagner.

River.	With Breadth in Metres.	Observer.	Number of Measured Curves.	Depth of Mean Velocity. Arithmetic Mean of the slightly varying values.
	Metres.			
Weser	80	von Wagner	6	0·587
Elbe	114	"	7	0·594
Rhine	215	"	4	0·589
"	215	"	1	0·600
Oker	14	"	3	0·600
Elbe	120	Harlacher	9	0·620
"	120	"	10	0·598
"	120	"	9	0·580
Danube	425	"	15	0·599
Mississippi		Humphreys and Abbot	..	0·580
Ganges Canal		Cunningham	46	0·620

The arithmetic mean of these eleven values was 0·597.

If the question was simply to ascertain the discharge for an engineering purpose, then he thought it would be accurate enough to take five to seven verticals on the cross-section of a river and to ascertain the velocity on each vertical at 0·6 of the depth, measured from the water-surface.

To this he would add that the maximum velocity in the sixty-four curves, given in the Table, varied in position, from the surface (for example in the Weser), down to 0·25 of the depth from the surface (as in the Rhine), and yet the proportionate depth of the mean velocity differed but little.

XVII. Perfectly agreeing with these explanations, he begged leave to add the following remark. He was of opinion that at the bottom the velocity must be zero. From that point the velocity increased rapidly. The idea of "bottom velocity" was therefore very indefinite. In practice it was frequently stated, and its value was taken as equal to O N (Fig. 8), obtained by prolonging the normal parabola A M to meet the bed, although it was proved that the velocities in the part L O decreased quicker than was given by the parabolic law. In his opinion "bottom-velocity" meant only that velocity which existed at the height of the centre of gravity of the boulders, on which the local average size of the boulders depended. If at any place the boulders had an average size of 10 centimetres, then there the bottom-velocity was greater than at a place where the bottom was formed of sand of an average diameter of 2 millimetres (Fig. 9). It was therefore fruitless to seek a relation between the bottom and other velocities. As to the measurement of bottom-velocity, the current-meter was

Prof. von Wagner.

not very suitable because of the distance which must be allowed for the rotation of the screw-blades. By Darcy's gauge, measurements could be made at 1½ centimetre above the bottom, provided it was level. This was shown by the curve Fig. 86 on Plate 8 in the "Hyd. Untersuch.," where, at each of the given depths, the difference of the water-columns had been observed thirty times.

Fig. 10.

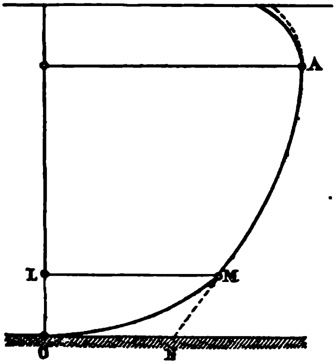
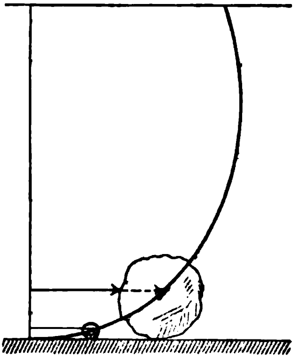


Fig. 11.



XVIII. Here the remarks confirmed the opinions in "Hyd. Untersuch.," pp. 40, 41, on the approximate conformity of the horizontal velocity-curve with the form of the wetted border, which was illustrated in Plate 8, Figs. 87 to 93. It would be seen that the conformity (which he had in vain sought to express algebraically) ceased in the same river with increase of discharge, (Figs. 92, 93), and besides differed near the shores (Figs. 87, 89, 93), in analogy with the alteration of the vertical curves.

XIX. He considered Harlacher's the simplest and most accurate method of determining the volume of discharge. It was explained in "The Measurements of the Elbe, Danube, &c.," by H. R. Harlacher, 1881. He had used the same method in his "Hyd. Untersuchungen," p. 18.

XX. The valuable work of the Author confirmed what Professor von Wagner had said, on p. 33 of the "Hyd. Untersuch.," about the degree of reliability of different formulas, and the statements in the Table, p. 32.

As the measurements in the Ganges Canal had been conducted with great care, it seemed desirable to make use of them to test two laws given in the "Hyd. Untersuch." If a continued confirmation of these laws resulted, then it would be of importance to practical engineering.

Prof. von Wagner.

Let,

v = mean velocity of the whole cross-section.
c = maximum surface-velocity in the whole transverse width.
v_s = velocity at the centre of figure of a cross-section.

(I.) *Relation between v and c.*—In twenty-four different gaugings of small and large streams, he had found

$$v = a\,c + b\,c^2.$$

The constants a and b were only discussed in a preliminary way in "Hyd. Untersuch." In his article in the "Deutsche Bauzeitung" (No. 82, 1882), more exact values had been obtained by the method of least squares, and these gave

$$v = 0{\cdot}705\,c + 0{\cdot}01\,c^2.$$

From an article in the "Deutsche Bauzeitung," it would be seen that another gauging (of the Elbe, above Hamburg) agreed well with this equation; the calculated value was only 2½ per cent. greater than that obtained by measurement. Should this equation prove sufficiently accurate, then the engineer would only need to measure c by surface floats, in order to get a good approximation to the value of v, and from that the discharge.

(II.) *Relation between v and v_s* (p. 38 of "Hyd. Untersuch.")—For this an equation had been formed from seven rivers, or, adding the gauging of the Elbe above Hamburg, from eight rivers. Using the method of least squares to determine the constants, the equation was—

$$v = 0{\cdot}738\,v_s + 0{\cdot}05\,v_s^2.$$

The gauging of the Elbe above Hamburg gave by this equation $v = 1{\cdot}16$ metre, while by direct measurement $v = 1{\cdot}17$. However, eight examples for determining the constants were far too few, although the differences were only—

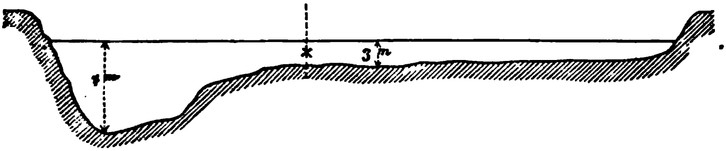
1·10	1·10	0·10	per cent. too great.
2·30	1·00	0·70	,, ,, ,, small.
		0·00	

Further the equation did not apply in cross-sections such as that of the Danube, Fig. 12, given by Harlacher. He presumed the equation would only apply to pretty regular and symmetrical cross-sections. Should the equation be confirmed it would only be necessary to determine the centre of figure of a cross-section, and

Prof. von Wagner.

measure the velocity at that single point. He wished that both equations might be tried by the Ganges Canal results.

FIG. 12.



Major Cunningham.

Major **ALLAN CUNNINGHAM** observed, in reply to the correspondence, that the points noted by Mr. Flamant about comparison of levels of still and running water in free communication, and as to non-existence of surface convexity in permanent regimen, and non-existence of transverse surface-flow, were of high scientific interest. Further direct experiment was very desirable. As to the mode of weighting the velocity-data of a vertical curve, objected to by Mr. Gordon, it was known that with double-floats the velocity-data increased in accuracy from the bed upwards, so that the weights assigned should also increase from the bed upwards; but at what rate was of course unknown, and therefore a matter of judgment. No stress was laid on the weighting used. Admitting with Mr. Leslie that the action of the air and wind on the projecting portions of the floats used might have exaggerated the observed depression of the maximum velocity-line, still all instruments alike agreed in showing this depression as an existing fact. The irregularity of the bed of the Ganges Canal was not greater than that of most so-called natural streams, as for instance the Thames, in which lumps, bars, hollows, &c., of 1 foot or 2 feet depth were common: on the vertical scale used in the plates (Vol. II. of the Roorkee Work) these features were enormously exaggerated. Most of the data asked for or suggested as requisite by several of his critics had been actually printed in great detail in Vols. II. and III. of the original work. Whilst acknowledging Du Buat's great advance in hydraulics as previously known, it could not be admitted, as said by Mr. Neville, that "nothing better had been done since" in way of a mean-velocity formula. The Darcy-Bazin and the Kutter formulas were both important advances. Also Du Buat's "rational theory," quoted by Mr. Neville, was merely a highly general principle: the detail was still wanting to enable mathematical investigation to be properly applied to the flow of water, and in this sense a "rational theory" was still wanting. If Professor von Wagner's conclusions, p. 88 (*a*), Major Cunningham. that the vertical velocity-curve deviated greatly from a parabola near both surface and bed were correct, and that the real bed-velocity was zero, it would be right to give up the use of the parabola approximation. Lastly, Major Cunningham desired to thank the several speakers and writers for the great value of their remarks to him in his special research; his only regret now was that these remarks were not available to help him when the experiments were being made.

21 November, 1882.

JAMES BRUNLEES, F.R.S.E., Vice-President,
in the Chair.

The discussion upon the Paper on "Recent Hydraulic Experiments," by Major Allan Cunningham, occupied the whole evening.

28 November, 1882.
JAMES BRUNLEES, F.R.S.E., Vice-President,
in the Chair.

(*Paper No. 1842.*)

"American Practice in Warming Buildings by Steam."[1]

By the late Robert Briggs, M. Inst. C.E.

The application of Steam to the Warming of Buildings in the United States originated with the late Mr. Joseph Nason, of Boston and New York, who died six or seven years ago. He was not only the first to make the attempt, but was also the originator, improver, and adapter of much that is essential, and now implicitly followed, in the general arrangement and details of the apparatus employed. He enjoyed the advantage of having been for a short time a pupil of Jacob Perkins in London, about 1840; and his earliest endeavour in America was to adapt the Perkins system of hot water inside small tubes for meeting the severity of the climate of America. The large extent of warming surface and the great strength presented by steam apparatus constructed of small and comparatively inexpensive wrought-iron tubes, and the facility thereby afforded for transmitting heat in any direction from a central source, are merits which led to so rapid a development of this system of warming, that by 1860, or in less than twenty years, there were already many hundred establishments throughout America for the manufacture of the apparatus. With the maturing of the system are associated the names of Mr. J. J. Walworth, of Boston, brother-in-law and partner of Mr. Nason; Mr. Gregg, of New York; Mr. J. O. Morse, of New York, by whom, amongst other improvements, was introduced the method of closed circulation for working with steam below atmospheric pressure; Professor Mapes, of New York, who supplied a reliable steam-trap; Mr. Miles Greenwood, of Cincinnati, to whom the Author believes the arrangement is due of a coil or nest of tubes connected by return-bends; and Mr. Thomas T. Tasker, of Philadelphia, who introduced the first closed apparatus. For himself the Author may claim to have established certain characteristic

---

[1] The discussion upon this Paper occupied part of two evenings.

shapes and dimensions now universally adopted for the "globe" stop-valves and for the fittings or couplings of the tubes.

Wrought-Iron Welded Tubes.—In the construction of the apparatus for warming by steam, the prevailing practice in America is to employ wrought-iron welded tubes, not only for the mains, but also to a large extent for the radiating surfaces that diffuse the heat. The separate lengths of tubes are connected by wrought-iron couplings, when in the same straight line; and when not so, by cast-iron elbows, tees, branch-pieces, and return-bends. The so-called coils or radiators usually employed for diffusing the heat are compact nests of tubes, sometimes arranged vertically by having their bottom ends screwed into a cast-iron box, and at other times placed horizontally and connected together by branch-tees and return-bends.

In the use of these tubes, the essential feature of practical importance is the employment of tapering screw-threads, externally upon the tube-ends and internally within the sockets of the couplings or fittings, as the means of securing durable steam-tight joints which can be readily made or unmade. The system of taper screw-threads for the tube connections is believed by the Author to have been originated by Mr. Nason, with whom he became associated in 1846; at that time the threads were cut in a lathe.

Where any two metallic surfaces are to form a steam-tight joint, they must be brought together into complete contact, either with each other or with some elastic or yielding packing interposed between them; and the force compressing them together must be sufficient both to obliterate any inequalities in the surfaces themselves, and also to withstand any internal pressure tending to part them. In the case of the machine-finished surfaces of the external and internal taper screw-threads forming the tube joints, the required contact is no doubt mainly effected by the yielding of the metal of the tube under compression and of the socket under tension; while the thread itself serves to maintain the joint against the internal disruptive steam-pressure.

A screw-thread, either external or internal, cut or tapped by means of the usual workshop appliances, is far from possessing absolute accuracy in radius, either for the top of the thread or for the bottom of the groove; and presents also the defect known as "drunkenness," or want of uniformity in its inclination or pitch. In making a steam-tight joint, these imperfections have to be overcome by the application of force. The thread has also defects in angular form, arising from imperfections in the dies or taps, and from the metal having been torn instead of being cut clean in the formation of the thread. These defects are remedied by the use of a paste, made of white-lead ground in linseed oil and mixed with about an equal quantity of dry red-lead. This material acts both as lubricant and as packing in making the joint; it fills all interstices in screwing the joint up, and sets hard on the first application of steam heat, or within a short time if left cold.

The leverage of the pipe-tongs or wrench, employed to screw the tube up, is largely multiplied by the action of the screw itself in converting the force into the longitudinal direction; and the tube end being moreover shaped as a conical plug of slight taper, the longitudinal force results in a greatly intensified pressure of contact between the threads of the tube and its socket. For example:—the ordinary pitch is 11½ threads per inch for a tube of 1 inch nominal diameter inside or 1·31 inch actual diameter outside, and the taper of both the tube end and the inside of the socket is 1 in 32 to their axis. After the tube has been entered into the socket as far as it can readily be screwed up by hand, one quarter turn more with the pipe-tongs is generally sufficient to make the joint steam-tight. In this quarter turn of the tube the difference of diameter is in the ratio of 1 : 11½ × 16 × 4, and is therefore equal to 1-736th of an inch. Meanwhile the hand, acting on the tongs at a radius of usually about 16 inches, will move through an arc of 25 inches, giving thus an effective leverage of say 18,000 times. The bursting pressure thereby produced within the socket, or the compression upon the tube end, is concentrated on about ½ inch length of the socket or tube, and the mean diameter is there about 1¼ inch; whence the pressure per square inch tending to burst the socket is roughly about 11,000 times the manual force exerted on the tongs, if the frictional resistance to turning be neglected.

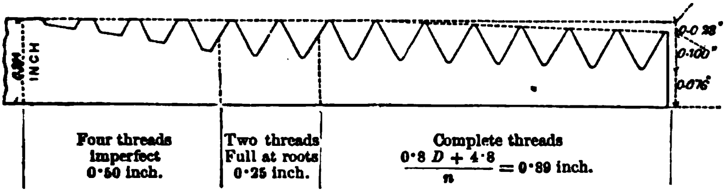
In practice it is really by the frictional resistance to turning that the limit of bursting strain actually thrown upon the cast-iron socket in screwing up is determined. For supposing the limit be taken to be a radial pressure of say 5,000 lbs. per square inch, it is seen from the foregoing that, after overcoming the frictional resistance to turning, an insignificant ½ lb. would be all the extra force required on the pipe-tongs for throwing this great bursting strain upon the socket. Allow now 15 per cent. as the coefficient of friction—a value not unreasonable when it is borne in mind that points of metal are brought into contact, and that the great pressure between them destroys the continuity of the imperfect lubricant. Then neglecting the screw-thread, and regarding the tube end and the casting as a plain conical plug and corresponding socket, the resistance to turning, offered by the ½-inch length of tube-end of 1¼ inch mean diameter, is 5,000 × 0·15 × 3·1416 × 1¼ × ½ = 1,400 lbs. acting at a radius of ⅝ inch, which is equivalent to about 55 lbs. exerted on the handle of the tongs of 16 inches radius. A skilled workman judiciously keeps well within this limit in the force which he exerts; and the cast-iron fittings being made according to the proportions hereafter given, the thickness of metal left outside the thread after tapping them is amply sufficient to stand the tensile strain so exerted, which rises probably to 10,000 or 12,000 lbs., say 5 tons per square inch of section of metal. In practice moreover it is found that a joint can safely be screwed up with the tongs through even one whole turn, instead of only a quarter turn; the wrought-iron tube-end yields permanently under the compression, while the limit of elasticity of the cast-iron socket is not exceeded, and hence neither metal has its strength impaired. Complete facility is thus afforded for leading off bends or branches in any desired angular position.

The taper employed for the conical tube-ends is uniform with all makers of tubes or fittings,—namely an inclination of 1 in 32 to the axis. Custom has established also a particular length of screwed end for each different diameter of tube. Tubes of the several diameters are kept in stock by manufacturers and merchants, and form the basis of a regular trade in the apparatus for warming by steam. A knowledge of all these particulars is therefore essential for designing apparatus for the purpose. The ruling dimension in wrought-iron tube work is the external diameter of certain nominal sizes, which are designated roughly according to their internal diameter. These nominal sizes were mainly established in the English tube trade between 1820 and 1840, and certain pitches of screw-thread were then adopted for them, the coarseness of the pitch varying roughly with the diameter, but in an arbitrary way utterly devoid of regularity. The length of the screwed portion on the tube end varies with the external diameter of the tube according to an arbitrary rule-of-thumb; whence results, for each size of tube, a certain minimum thickness of metal at the outer extremity of the tapering screwed tube-end. It is the determination of this minimum thickness of metal for the tapering screwed end of a wrought-iron tube which constitutes the question of mechanical interest.

A longitudinal section of the tapering tube-end, with the screw-thread as actually formed, is shown double full size in Fig. 1 for a nominal 2½-inch tube, that is, a tube of about 2½ inches internal diameter, and 2⅞ inches actual external diameter.

FIG. 1.

Thread of 2½ inch tube. Enlarged double full size.



The thread employed has an angle of 60°; it is slightly rounded off both at the top and at the bottom, so that the height or depth of the thread, instead of being exactly equal to the pitch, is only four-fifths of the pitch, or equal to $0\cdot8 \times \frac{1}{n}$, if n be the number of threads per inch. For the length of tube-end throughout which the screw-thread continues perfect, the empirical formula used is $(0\cdot8\,D + 4\cdot8) \times \frac{1}{n}$, where D is the actual external diameter of the tube throughout its parallel length, and is expressed in inches. Further back, beyond the perfect threads, come two having the same taper at the bottom, but imperfect at the top. The remaining imperfect portion of the screw-thread, furthest back from the extremity of the tube, is not essential in any way to this system of joint; and its imperfection is simply incidental to the process of cutting the thread at a single operation. From the foregoing it follows that, at the very extremity of the tube, the diameter at the bottom of the thread $= D - \left[\frac{2 \times (0\cdot8\,D + 4\cdot8)}{32\,n} + \frac{2 \times 0\cdot8}{n}\right] = D - (0\cdot05\,D + 1\cdot9) \times \frac{1}{n}$. The thickness of iron below the bottom of the thread, at the tube extremity, is empirically taken to be $= 0\cdot0175\,D + 0\cdot025$. Hence the actual internal diameter d of any tube is found to be, in inches,

$$d = D - (0\cdot05\,D + 1\cdot9) \times \frac{1}{n} - 2 \times (0\cdot0175\,D + 0\cdot025)$$

$$\text{or } d = 0\cdot965\,\mathrm{D} - 0\cdot05\,\frac{\mathrm{D}}{n} - \frac{1\cdot9}{n} - 0\cdot05.$$

For the various sizes of tubes, ranging from ⅛ inch to 10 inches nominal internal diameter, with their corresponding numbers of screw-threads per inch, the actual internal diameter d is expressed by the following Table I. in terms of the actual external diameter D.

TABLE I.

Diameters of Wrought-Iron Welded Tubes for Warming by Steam.

Nominal Internal Diameter of Tube.	Number of Screw-Threads per inch.	Actual Internal Diameter *d* in terms of actual external diameter *D*.
Inches.	No.	Inches.
⅛	27	$d = 0{\cdot}9631\ D - 0{\cdot}1204$
¼ and ⅜	18	$d = 0{\cdot}9622\ D - 0{\cdot}1556$
½ and ¾	14	$d = 0{\cdot}9614\ D - 0{\cdot}1857$
1 1¼ 1½ and 2	11½	$d = 0{\cdot}9607\ D - 0{\cdot}2152$
2½ to 10	8	$d = 0{\cdot}9587\ D - 0{\cdot}2875$

The figures derived from this statement which are of importance for practical use are presented in detail in the accompanying Table II. in a convenient order for reference.

The number of screw-threads per inch for the several sizes of tubes is here accepted from customary usage. It is the workman's approximation to the pitch practically desirable, and much reluctance must consequently be felt in calling it in question. Still it would have been better to investigate the general case upon the basis of a pitch ranging in closer accordance with the range of tube diameter. Thus the nominal ½-inch tubes might have had 16 threads per inch; ¾-inch, 14 threads; 1 and 1¼-inch, 12 threads; 1½ and 2 inches, 11 threads; 2½ to 3½ inches, 10 threads; 4 to 6 inches, 8 threads; 7 to 9 inches, 7 threads; and 10 inches, not more than 6 threads per inch. The existing numbers of threads however, as given in Tables I. and II., are now too well established to be disturbed: at all events they must be taken in any statement of present practice.

The smaller sizes of tubes, up to and including 1¼ inch nominal inside diameter, are butt-welded, and are proved by hydraulic pressure to 300 lbs. per square inch. The larger sizes, commencing with 1½ inch, are lap-welded, and are proved to 500 lbs. per square inch by hydraulic pressure. The question of butt-welding or lap-welding is simply one of economy in manufacture: it is not easy to make lap-welded tubes of less than 1½ inch inside diameter, while it is cheaper to make lap-welded than butt-welded tubes of that size and upwards. The proving pressures here given are far below the ultimate strength of sound welded tubes; and when required the test is increased to double or treble these pressures.

Couplings.—The internal threads in the sockets or fittings, by which the tubes are connected together, require the same accuracy of work-

TABLE II.

Standard Dimensions of Wrought-Iron Welded Tubes for Warming by Steam.

Diameter of Tube.			Thickness of metal.	Circumference.		Length of Tube per square foot of		Transverse Area.		Length of tube to contain 1 cubic foot.	Weight per foot of length.	Screwed Ends.	
Nominal Inside.	Actual Inside.	Actual Outside.		Inside diameter.	Outside diameter.	Inside surface.	Outside surface.	Inside diameter.	Outside diameter.			Number of Threads per inch.	Length of Perfect screw.
Inches.	Inches.	Inches.	Inch.	Inches.	Inches.	Feet.	Feet.	Sq. Inches.	Sq. Inches.	Feet.	Lbs.	No.	Inch.
1/8	0·270	0·405	0·068	0·848	1·272	14·150	9·440	0·0572	0·129	2,500·00	0·243	27	0·19
1/4	0·364	0·540	0·088	1·144	1·696	10·500	7·075	0·1041	0·229	1,385·00	0·422	18	0·29
3/8	0·494	0·675	0·091	1·552	2·121	7·670	5·657	0·1916	0·358	751·50	0·561	18	0·30
1/2	0·623	0·840	0·109	1·957	2·652	6·130	4·502	0·3048	0·554	472·40	0·845	14	0·39
3/4	0·824	1·050	0·113	2·589	3·299	4·635	3·637	0·5333	0·866	270·00	1·126	14	0·40
1	1·048	1·315	0·134	3·292	4·134	3·679	2·903	0·8627	1·357	166·90	1·670	11½	0·51
1¼	1·380	1·660	0·140	4·335	5·215	2·768	2·301	1·496	2·164	96·25	2·258	11½	0·54
1½	1·610	1·900	0·145	5·061	5·969	2·371	2·010	2·038	2·835	70·65	2·694	11½	0·55
2	2·067	2·375	0·154	6·494	7·461	1·848	1·611	3·355	4·430	42·36	3·667	11½	0·58
2½	2·468	2·875	0·204	7·754	9·032	1·547	1·328	4·783	6·491	30·11	5·773	8	0·89
3	3·067	3·500	0·217	9·636	10·996	1·245	1·091	7·388	9·621	19·49	7·547	8	0·95
3½	3·548	4·000	0·226	11·146	12·566	1·077	0·955	9·887	12·566	14·56	9·055	8	1·00
4	4·026	4·500	0·237	12·648	14·137	0·949	0·849	12·730	15·904	11·31	10·728	8	1·05
4½	4·508	5·000	0·246	14·153	15·708	0·848	0·765	15·939	19·635	9·03	12·492	8	1·10
5	5·045	5·563	0·259	15·849	17·475	0·757	0·629	19·990	24·299	7·20	14·564	8	1·16
6	6·065	6·625	0·280	19·054	20·813	0·630	0·577	28·889	34·471	4·98	18·767	8	1·26
7	7·023	7·625	0·301	22·063	23·954	0·544	0·505	38·737	45·663	3·72	23·410	8	1·36
8	7·982	8·625	0·322	25·076	27·096	0·478	0·444	50·039	58·426	2·88	28·348	8	1·46
9	9·000	9·688	0·344	28·277	30·433	0·425	0·394	63·633	73·715	2·26	34·077	8	1·57
10	10·019	10·750	0·366	31·475	33·772	0·381	0·355	78·838	90·762	1·80	40·641	8	1·68

Taper of conical tube-ends, 1 in 32 to axis of tube.

In tubes for heating water or steam, the surface in contact with the products of combustion is to be taken as the effective heating surface, whether it be the inside or the outside of the tubes. But in tubes for heating liquids by steam, for superheating steam, or for transferring heat from one liquid or gas to another, the mean between the inside and outside surfaces is to be taken as the effective surface. In warming by steam, the outside surface exposed to the air is to be taken.

manship as the external threads on the tube ends. In practice it is found that the straight couplings or sockets, connecting tubes in the same straight line, may be made of wrought iron, and may be tapped parallel instead of taper; they are sized to screw freely upon the small extremity of the tapering tube-end up to as far as half the length of the perfect thread on the tube; and they then become stretched by the force exerted in screwing them upon the remaining half length of larger diameter, and form their own taper to fit the tube. To allow for clearance between the ends of the two tubes united by the coupling socket, the length of the coupling is made as nearly 2½ times the length of the perfect-threaded tube-end as will suit the width of iron from which the coupling is manufactured. The minimum thicknesses of metal for the several sizes of couplings previous to tapping are given in Table III., with their corresponding diameters and lengths.

TABLE III.

Dimensions of Straight Sockets for Coupling Wrought-Iron Tubes.

Diameter of Tube.		Diameter of Socket before tapping.		Thickness of Socket before tapping.		Length of Socket.
Nominal Inside.	Actual Outside.	Outside.	Inside.	Actual.	Birmingham Wire Gauge.	
Inches.	Inches.	Inches.	Inches.	Inch.	No.	Inches.
⅛	0·405	0·551	0·333	0·109	12	* ⅞
¼	0·540	0·699	0·431	0·134	10	1
⅜	0·675	0·834	0·566	0·134	10	1
½	0·840	1·011	0·699	0·156	8½	1 1/16
¾	1·050	1·239	0·909	0·165	8	1⅜
1	1·315	1·527	1·143	0·192	6½	1¾
1¼	1·660	1·893	1·487	0·203	6	1¾
1½	1·900	2·166	1·726	0·220	5	*2
2	2·375	2·677	2·201	0·238	4	*2¼
2½	2·875	3·190	2·622	0·284	2	2¾
3	3·500	3·870	3·245	0·313	..	3
3½	4·000	4·404	3·724	0·340	0	3⅛
4	4·500	4·992	4·242	0·375	00	3¼
4½	5·000	5·491	4·741	0·375	00	3⅜
5	5·563	6·186	5·311	0·438	..	3½
6	6·625	7·236	6·361	0·438	..	3¾
7	7·625	8·358	7·358	0·500	..	4
8	8·625	9·489	8·364	0·563	..	4¼
9	9·688	10·539	9·414	0·563	..	4½
10	10·750	11·720	10·470	0·625	..	5

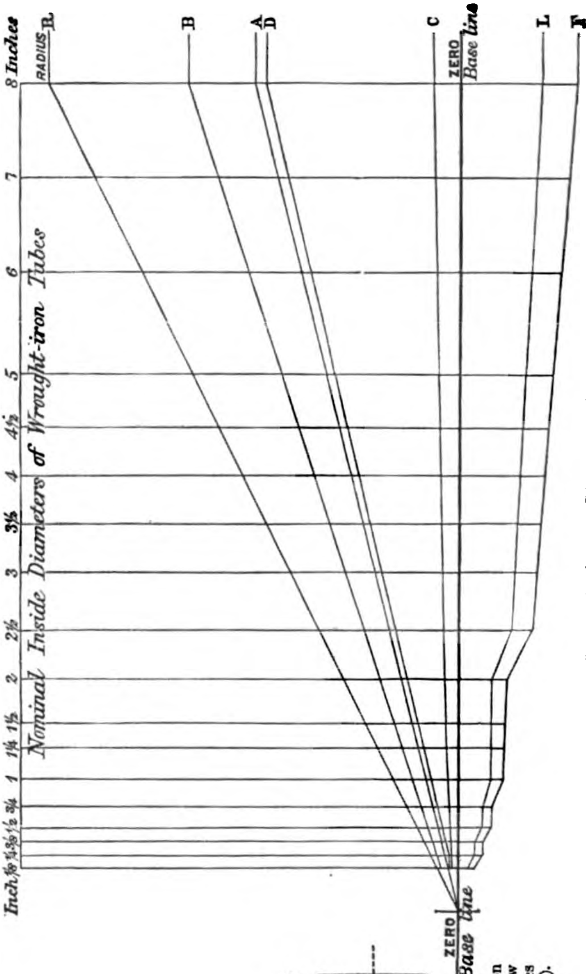
* The three lengths marked by an asterisk are increased disproportionately for appearance.

This parallel tapping is not altogether satisfactory for the straight couplings for tubes above 2½ or 3 inches nominal diameter, and the joints so made are not reliable if screwed up with the ordinary tongs; for elbows or tees the method is very unsatisfactory. Tapering threads are usual in the wrought-iron sockets for the "oil tubing" in petroleum wells, and for the "line pipe" through which the petroleum is conveyed; these tubes range from 5 to 10 inches in diameter, and are subjected to pressures of 1,200 to 1,600 lbs. per square inch; the tubes themselves are frequently burst by the pressures exceeding these, but the joints are perfectly reliable and unyielding. In the original construction of taper joints by Mr. Joseph Nason, the sockets of all sizes were made taper; the subsequent departure from this practice was a matter of cheapness in manufacture, and has never been a mechanical success.

The great power required for tapping a tapering thread in wrought iron, and the extra cost of making wrought-iron elbows and tees &c., have led to the employment of cast iron as the material for the fittings or couplings. Careful study having been bestowed upon the reduction of the material to the smallest quantity requisite, and upon the production of the cored castings with the least amount of preparatory work, these cast-iron fittings have now become universal for all joints of wrought-iron tubes, excepting only the straight couplings already mentioned. The scale drawn one-quarter full size in Fig. 2 gives the exact dimensions for all parts of every possible elbow, tee, cross, or branch, for tubes of $\frac{1}{8}$ inch up to 8 inches nominal inside diameter. With these dimensions the least quantity of material, consistent with uniformity of strength, is here employed, and is arranged in the most compact form. A longitudinal section is shown in Fig. 3 of a 1¼-inch elbow, that is, the elbow suitable for a nominal 1¼-inch tube. The dimensions for this section are obtained from the scale by measuring, along the vertical line headed 1¼ inch, the height of ordinate intercepted between the horizontal base or zero line and the slant line marked by the letter corresponding with that on the section. In the two orifices, which have to be tapped subsequently for screwing upon the tube-ends, the diameter (twice the dimension marked D in the section, Fig. 3) is cored just so much smaller than the smallest diameter of the root of the thread on the tube-end as will allow for removing the scale of hard iron from the casting by means of a drill; after which a taper tap, corresponding with the external thread on the tube-end, produces the internal thread in the socket, cutting with the proper taper a perfect thread inside the orifice throughout the whole of its length, marked L in the section. There are therefore, excepting in special cases, no imperfect threads in the cast-iron fittings or sockets.

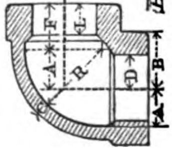
It may be well to explain, in regard to the construction of the scale shown in Fig. 2, that the ordinates, though here headed for convenience with the nominal inside diameters of the tubes, are spaced from the zero point at horizontal distances (half full size) equal to the actual outside diameters as given in the foregoing

FIG 2.



Quarter-size Scale for Dimensions of Cast-Iron Fittings.

FIG. 3.



Longitudinal Section of Cast-Iron Elbow for 1¼-inch Tubes (before tapping). Quarter full size.

Table II. Of the slant lines, only two pass through the origin or zero point:—namely, the line for the internal radius R of the back of the elbow, and the line giving the dimension A which is exactly half the radius R. Of the remaining lines, those giving the three dimensions B, D, C, intersect the base line a little to the left of the origin or zero point: which means that each of the three dimensions given by these lines contains some small increment in excess of the dimension that would be strictly proportional. For instance, with regard to the thickness C at the back of the elbow, it is evident that, if the metal in any casting, whether small or large, thin or thick, were truly homogeneous throughout both its skin and its interior portion, this thickness should then be strictly proportional to the diameter or radius of the bore; but for small castings a certain allowance of extra thickness is needed, in order to ensure soundness, as well as to compensate for the increased brittleness of thin cast-iron. There will however be some size of fitting, for which the normal strength of metal may safely be assumed, and for which therefore no extra thickness will be needed. On this assumption the thickness for the largest size is set out at the end of the scale furthest from the origin; while nearest to the origin is set out the thickness proper for the smallest size, in which the increment is at its maximum; and the slanting straight line drawn between these two extremes gives for intermediate sizes an increment which varies inversely as the diameter or radius. Needlessly complicated formulas, involving higher or lower powers of the diameter, are thus replaced by the simple straight line. In the two bottom lines L and F, pertaining to the screwed portions of the fitting, which are a function of the pitch, the irregularities of slope are due to the fact already commented upon, that the pitch of the screw-thread for different diameters of tube proceeds by a medley of arbitrary jumps, instead of by a steady progression concordant with that of the tube diameter.

Similar scales, but much more elaborate, furnish the dimensions for the valves and other details in the construction of steam-heating apparatus. The leading example already given will suffice however as an illustration of the method.

It might seem that too much prominence has here been given to minor details, in dwelling at such length upon the screw-threads and the exact proportions of tubes and fittings; but it can be unhesitatingly asserted that facility of construction, economy of material, and careful elaboration of detail, have been largely concerned in the establishment in America of an industry almost unknown in Great Britain, and of which the history has hitherto remained almost wholly unrecorded. Indeed the only book in the English language on heating by steam, until the publication in America of an elementary work a few months ago, was that of Robertson Buchanan, 1811-1814. Beyond the catalogues and circulars of manufacturers and dealers, little or no practical information has appeared in print; while a whole class of skilled workmen, and nearly forty years of experience, have developed the practicability of the system, and extensive usage has accustomed the entire American community to its attendant discomforts and annoyances.

Boilers.—Most frequently, and in particular for large apparatus, the steam for warming in America is supplied by one or more horizontal tubular or Seguin[1] boilers, set in brickwork. For a small extent of warming apparatus the vertical tubular boiler with internal firebox is possibly the one most usual. For medium and large apparatus the original Seguin boiler is adopted, which consists of a horizontal cylindrical shell, containing tubes in the lower half, with a steam dome on the top; the tubes are arranged in vertical and horizontal rows, not alternating or zigzag; and the best practice is to place a manhole in the front end over the firedoor, and beneath the tubes. The boiler is fired underneath, and the products of combustion return through the tubes from the back to the front, and then pass back again over the top of the boiler, which is covered in with brickwork; the temperature of this top flue rarely exceeds 400° Fahrenheit. The fuel used is anthracite coal, yielding 8 to 12 per cent. of ash; and when well supplied with air it evaporates from 8½ to 9 lbs. of water per lb. of coal. The boiler fittings, gauges, valves, pipes, &c., are such as are usual for boilers in England: only, as they are rarely made by engine-builders, but by the "steam-fitting" maker, it can be claimed for them that they are generally more sightly, as well as better and less expensive, than in England. Dampers and damper-regulators are in general use, the latter being constructed with elastic diaphragms arranged to be loaded for any desired temperature, and often with some automatic arrangement for changing the load to meet changes of temperature outside or inside the building warmed. Where the whole of the circulation through the warming apparatus is entirely closed from communication with the atmosphere, the boiler pressure is sometimes allowed to fall below atmospheric pressure, to as low as 140° temperature of the steam.

One other boiler at least has successfully stood the severe test of

---

[1] Introduced in France about 1826 to 1830 by Mr. Seguin, engineer of the St. Étienne and Lyons Railway; and in America about 1844 by Mr. Nason.

competition with the Seguin boiler, namely that known as the Babcock and Wilcox water-tube circulating boiler,[1] having a horizontal steam-drum or separator overhead. This boiler was tried by Mr. Nason in nearly its present form, but with smaller tubes, the joints of which failed as made by him.

Either of these boilers is practically safe from disastrous explosion. Out of the many thousands of Seguin boilers which have now been in use from one year to forty years, only two or three well-authenticated instances have occurred of violent explosion, and none, so far as the Author is aware, have involved loss of life. Of all the stationary boilers that are employed in the United States for any purpose whatsoever, perhaps one half are of the Seguin type. The deterioration of all heating-apparatus boilers is more serious from rusting in summer when idle than from wear in use. In fair service the duration of the shell of the horizontal tubular boiler is from thirty to forty years, at least where water is employed that does not produce scale or deposit. The wrought-iron flue-tubes cannot be considered to last more than seven or eight years, even with water of good quality.

The most serious defect in the Seguin boiler is the liability of the bottom plate, just over the fire-bridge, to "come down" or bulge outwards, or to become blistered, when strong firing is carried to the extent of burning 10 lbs. of coal or upwards per square foot of grate per hour. This accident is more likely to happen when the tubes are arranged zigzag in alternate rows, or are placed close together, the circulation of the water between them being impeded in either case; and the provision of a large water-space in the bottom of the boiler, such as results from the practice already mentioned of placing a manhole in the front end beneath the tubes, tends to obviate the risk. Recently a hollow framing or slab, made of boiler-plate, filled with water and connected with the boiler by circulating pipes, has been substituted for the front plate of the furnace containing the fire-door, and thus forms a permanent wall, serving also as a saddle to support the front end of the boiler. The details of brickwork setting, flues, and chimney, present no unusual features as carried out in America.

There are of course innumerable varieties of boilers and settings. The possible contortions of boiler-plates, tubes, and castings, are far from being exhausted; and the economy claimed for each fresh device is generally in proportion to its complication. Just now there is a crop of devices in America for the more com-

---

[1] Minutes of Proceedings Inst. C.E., vol. lxi., p. 379.

plete combustion of gases, after the plans generally of the late Mr. Charles Wye Williams, Assoc. Inst. C.E.; some of these, and others for burning such fuel as saw-dust, coal-dust, spent tan, &c., are as successful as they were thirty years ago.

For the fire-grates also of the boilers there is a revival of sundry shaking appliances; but for the American anthracite these schemes will speedily die out, since it resents the least disturbance after having been once ignited, and admits only the gentlest turning at the bottom for allowing the ash to run out like sand from an hour-glass. An improved grate however, made with wrought-iron fire-bars of square or triangular section, with their ends resting in bearings so that each bar separately can be rotated, seems really to have won favour for heavy firing, and must be mentioned with approval.

For the smaller sizes of warming apparatus there are several devices of magazines for holding a store of fuel, from which a continuous supply to the fire-grate is dragged down or allowed to run down upon the grate. Some of these magazines hold 4 to 8 tons of coal, which will last from one month to three months. It may be mentioned that American anthracite coal, having the strength of most English building stone, is got from the seam in massive blocks; these on reaching the pit bank are broken up into lumps, which for domestic use are screened to various sizes, ranging from as small as peas to as large as eggs, while the "furnace coal" for manufacturing purposes is sorted in still larger sizes.

Circulation of Steam for Warming.—There are two methods of warming with steam, one with live steam direct from the boiler, and the other with exhaust steam. These two are frequently carried out in combination, and in fact generally so where exhaust steam is used at all for warming. The circulation or distribution of the steam through the warming pipes is effected in an almost unlimited variety of ways, each possessing advantages for special cases. The cause producing the circulation throughout the pipes of the warming apparatus is solely the difference of pressure which results from the more or less rapid condensation of the steam in contact with the radiating surfaces; a partial vacuum of greater or less amount is thereby formed within the radiating portions of the apparatus, and the column of steam or of water equivalent to this diminution of pressure constitutes the effective head producing the flow of steam from the boiler, while the return current of condensed water is determined by the downward inclination of the pipes for the return course.

When using live steam direct from the boiler, the system of what is called closed circulation is carried out either with separate supply and return mains, both of which extend to the furthest distance to which the heat has to be distributed; or else with a single main, which answers at once for both the supply and the return, either with or without a longitudinal partition inside it for separating the outward current of steam supply from the return current of condensed water. In what is called the system of open circulation, a supply main conveys the steam to the radiating surfaces, whence a return main conducts the condensed water either into an open tank for feeding the boiler, or into a drain to run to waste, the boiler being then fed from some other source; in either case suitable traps have to be provided on the return main, for preserving the steam-pressure within the supply main and radiators. These two systems, in any of their modifications, may also be combined, as is most generally done in any extensive warming apparatus.

In connection with closed circulation, the exceptional character of the single-main distribution calls for some further remarks. The employment of a single main for both supply and return is restricted to buildings where the horizontal distances through which the heat has to be distributed are very short, but where the vertical distances are relatively great. These conditions occur more particularly when the warming is extended to several storeys, as in single dwellings or office buildings covering little ground.

There are three principal methods of single-main distribution. In the first, the main is devoid of any internal partition, and is carried up from the boiler at once to the highest point, without taking off from it on the way any distributing branches, all of which are led off from it afterwards in its descending course. Wherever in the ascending course, before the highest point is reached, it has to take an inclined instead of a vertical direction, it must rise at an inclination of not less than 1 in 30 to the horizontal, in order to let the condensed water run back in spite of the current of steam passing in the contrary direction. To ensure the size being large enough, the diameter for a vertical main should be taken at least equal to that furnished by Tables VII. to X. in the Appendix; but for an inclined main (rising 1 in 30) the diameter should be doubled. This first method however is much embarrassed by the air which gets entrapped within blind branches that offer no thoroughfare; the radiators are consequently not certain of getting heated, and the apparatus is apt to be noisy for the reasons subsequently explained.

The second method applies to distributions extending through considerable distances horizontally. Here it is more satisfactory to start with a primary circulation through separate inclined supply and return mains, in place of a single main however large or however steep; and to relegate the single-pipe system to a secondary rank, employing it for vertical branches led off from the primary supply main. The course of the primary supply main is a descent with proper slope to the remotest horizontal distance; and, immediately underneath each single-pipe branch rising vertically from it, a receptacle is provided in the primary main, large enough to catch all the condensed water returned from the branch; these receptacles are themselves drained back to the boiler by the return main. From the vertical single-pipe branches, short single offshoots are led off laterally to the radiators in the rooms of the different storeys. The air difficulty is avoided with much success by carrying each vertical branch first to the highest point required, free from all lateral offshoots; and supplying the several radiators from its descending or return course, by connections so arranged as to preclude lodgment of air.

In the third method of single-main distribution, which is possibly as satisfactory as either of the two preceding, a single upright pipe is employed, having a hoop-iron partition tightly inserted for separating the ascending from the descending current. Such a pipe need be no larger than prescribed by Tables VII. to X. in the Appendix.

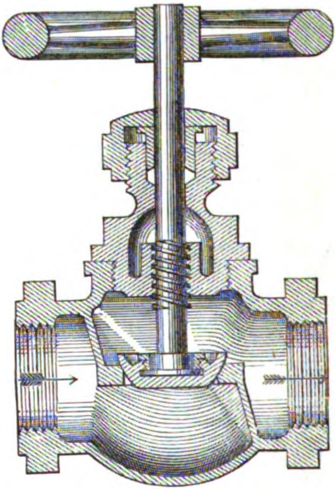
The system of closed circulation requires the boiler to be placed so low as will allow all the return pipes to drain freely back to it above its water-level. This condition has been modified mechanically by the automatic "Albany trap," a device frequently employed for lifting from a lower level part or all of the condensed water, and delivering it into the boiler: it is in fact a displacement pump. The same result has been attained by draining into a closed tank, placed low enough to accommodate all the return pipes, and made strong enough to stand the full boiler-pressure with safety; and then employing a steam-pump, either reciprocating or centrifugal, to raise the water from this tank to the proper level for enabling it to flow back into the boiler, the whole of the circulation being closed from communication with the atmosphere.

Steam mains and branches are apt to be noisy whenever any dipping bend or pocket in the pipes, or any recess in the fittings, allows water to accumulate, and to become cooled below the temperature of the steam supply. In such cases a rapid condensation occurs there, and the steam rushing in carries the water along with it; or sometimes two opposing currents of steam, rushing into the vacuum, meet each other; and characteristic noises of rattling and hammering are produced.

Where separate supply and return pipes are used, whether the system of circulation be open or closed, they should everywhere slope downwards in the direction of the current of steam or of water. A fall of ½ inch in 10 feet (1 in 240) has been found ample to provide against all sag of the pipes or other mechanical defects in the work, and to ensure silent working. When the levels at which the radiators have to be placed do not admit of this slope being continued in the so-called horizontal supply main, vertical breaks are made in the line by the insertion of pipes of larger diameter, which are trapped by check-valves or siphons into the return main. In any extended distribution by separate supply and return mains, the supply main should be connected to the return at the remote ends by the method of open circulation; and between any parallel or not very distant supply and return pipes, occasional drips should be provided, at intervals of say 600 to 1200 times the pipe diameter, siphoned off to prevent any short circuit. Branches upon the horizontal mains, whether supply or return, should be connected upon the top of the main, not at the sides or bottom. Freedom for expansion should be allowed by horse-shoe or S bends, or when practicable by elbows judiciously arranged. Expansion joints are an established fitting for warming apparatus; but their use is not to be approved except in emergency. Repairs are facilitated by substituting at frequent intervals along the mains, in place of some of the screw couplings, cast-iron flange joints, the flanges being screwed upon the tube ends. Main pipes should in all cases be either carried on rollers or suspended, to allow freedom for expansion without straining the joints. The Author does not attempt to describe completely all the minutiæ of detail in the construction of the mains for warming by steam; but simply notices some of the appliances commonly employed in America, which have been devised and worked out practically, and are regularly manufactured for general use.

Clothing of Steam Mains.—To prevent loss of heat, steam mains are protected by clothing, as described by Rumford in the last century. Felt is found to perish when applied direct to a surface as hot as 200° Fahrenheit; and Rumford's air-space, formed by enclosing the main within a rather larger casing of thin cast iron, or of sheet-iron either plain or tinned or zinced, is sufficient for enabling the felt to be employed as the clothing outside the casing. A covering of wire-netting has also been devised. Coats of porous terra cotta or of porous plaster answer, by their low conductivity, to save the felting put outside them. Outside the felt again is applied some suitable sheath or protecting covering that will stand the exposure. When thus clad, the loss incurred in carrying a steam main to any distance—either out of doors, or inside rooms, in passages, in cellars, or in culverts or flues either underground or above—is found to be less than 1 unit of heat for each 100 square feet of external surface of the main itself. The ratio which this loss bears to the total quantity of heat transmitted is exemplified for particular cases in Table XII. in the Appendix.

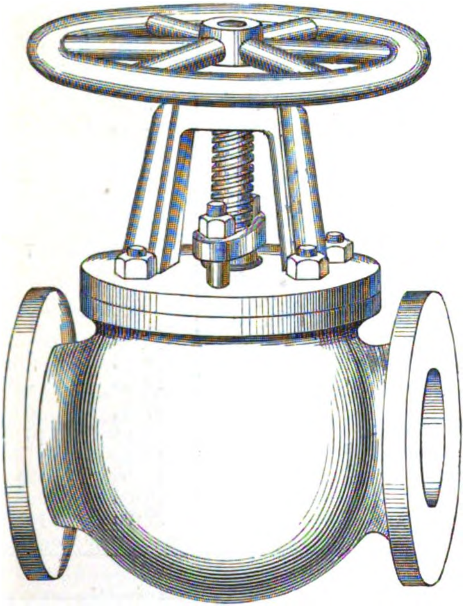
Fig. 4.



Tapped Globe Valve.

Steam Stop-Valves.—The steam stop-valves, known as "globe valves," are disk or poppet valves, worked by a screwed spindle, as shown in Figs. 4 and 5, which represent the "straight-way" make for insertion between two pipes in the same straight line. The annular seating upon which the disk closes is cast in line with the axis of the pipes, and in the middle of the globular or spherical body; a transverse diaphragm above the near end of the seating, and a corresponding wall crossing beneath its far end, close all thoroughfare excepting the aperture in the seating itself. This construction was introduced by the Author in 1849, and was immediately followed by all makers. The same globular form for the body has been adopted also, as a matter of symmetrical appearance, for the three makes of valve employed to unite pipes at right angles: the first, known as angle valves, unite two pipes, of which one is in line with the valve-spindle, and the other is at right angles to it; the second, called corner valves, unite two pipes at right angles to each other and both of them at right angles to the spindle; and in the third, or cross valves, two pipes in line with each other and at right angles to the valve-spindle are united to a third pipe in line with the spindle. The principal dimensions of straight-way globe-valves are given in Table IV. for the various sizes in use. In general the smaller valves, not exceeding 1½ inch in diameter of opening, are wholly of gun-metal; the larger are commonly and preferably made with cast-iron bodies and gun-metal fittings. The smallest valves, from ¼ inch up to ½ inch inclusive, have the disk solid with the spindle, and have an ordinary stuffing-box with external gland. Valves of ¾ inch and upwards have the disk loose from the spindle, and the spindle is made with a cheese-head let into a recess in the disk, and secured there by an annular nut; they have also a separate internal gland, tightened by a screwed

Fig. 5.



Flanged Globe Valve.

TABLE IV.

Dimensions of Straight-way "Globe" Valves.

SIZE. Diameter of Opening in seating.	BODY. Gun-metal or Cast-Iron.	NOZZLES. Tapped or Flanged.	Length over all.	Diameter of Flanges.	No. of Bolts in each flange.
Inches.			Inches.	Inches.	No.
¼	Gun-metal	Tapped	1·45		
⅜	do.	do.	1·78		
½	do.	do.	2·20		
¾	do.	do.	2·65		
1	do.	do.	3·30		
1¼	do.	do.	3·85		
1½	Gun-metal	do.	4·35		
	Cast-iron	do.	5·10		
2	Gun-metal	do.	5·30		
	Cast-iron	do.	5·90		
	Cast-iron	Flanged	5·75	6	4
2½	Gun-metal	Tapped	6·75		
	Cast-iron	do.	7·30		
	Cast-iron	Flanged	7·25	7	4
3	Gun-metal	Tapped	7·75		
	Cast-iron	do.	9·25		
	Cast-iron	Flanged	9·25	7½	4
3½	Cast-iron	Tapped	10·25		
	do.	Flanged	10·25	8	5
4	do.	Flanged	11·25	9	5
5	do.	do.	13·25	10	6
6	do.	do.	15·25	11	6
8	do.	do.	19·00	13½	8
10	do.	do.	23·00	16	10
12	do.	do.	27·00	19	10

cap, as shown in Fig. 4. The valve-spindles are screwed left-handed, with a double thread of square section; up to 3-inch valves the spindles are screwed to work inside the casing, as in Fig. 4; above that size the screwed portion is outside the casing, and works through a nut in a stool bolted on the casing, as in Fig. 5. In all the valves the disks and seatings have their surfaces of contact shaped spherical; and the disks are without wings to guide them. Above the 3-inch size the nozzles of the cast-iron bodies are generally flanged instead of tapped. This construction is particularly satisfactory for large valves, 10-inch and 12-inch; the last is the largest size usually kept in stock. The resistance presented by a globe-valve to a current flowing through it is assumed at half as much again as the resistance of a sharp right-angled elbow. The equivalent length of steam pipe representing the proper allowance to be made for each valve's resistance is given in Table XI. in the Appendix, for the several sizes of steam pipes.

Radiators for diffusing heat.—In respect to the radiating surfaces for the diffusion of the heat, there are three distinct classes of warming apparatus in use in America, according to the object to be effected.

First, there is the apparatus for warming rooms by so-called direct radiation; that is, by means of radiating surfaces exposed in the rooms themselves.

Secondly, there is the apparatus for what is sometimes called indirect warming, by means of currents of air: the heated surfaces are placed in a chamber, through which a limited supply of air is allowed to pass on its way into the room. In neither of these two methods is the warming accompanied by any systematic ventilation.

Thirdly, there is the apparatus for both warming and ventilating, arranged so that the warming shall take effect upon the whole supply of air admitted for ventilation.

The first and second methods are, with rare exception, employed for all dwellings, offices, and mills; the third is reserved for hospitals, asylums, public buildings, and the like. On the first and second plans, the warming apparatus is required to be capable of maintaining a comfortable warmth in the very coldest weather, when the outdoor temperature ranges as low as 15° below zero Fahrenheit; whence it is argued in favour of these two methods that in moderately cold weather their warming capacity will be so largely in excess as to admit of ample ventilation by opening windows or doors at pleasure. There are some who condemn altogether the first method or direct radiation, and strongly recommend the second or indirect, on the ground of the supply of fresh air secured by the warming currents in the latter method. But, as a rule, this supply of fresh air is inadequate; and, with the prevalent construction of apparatus, the temperature of the warming currents on issuing from the heating chambers is too high, and cannot be controlled; and the regulation of the warmth in the room has to be effected by opening wider or partially closing the hot-air inlet from the heating chamber into the room, thereby altering the admission of fresh air. On the score of ventilation, the second method is perhaps preferable to the first, because for any temperature of the external air the second method does certainly supply some admission of fresh air, however varying and inadequate. But in the estimation of the community at large, the first method—direct radiation from surfaces exposed in the room itself—offers the pre-eminent advantage that the warming of a room is effected with great certainty and rapidity, and is under the immediate control of the occupant. Direct radiation has also the merit of requiring the smallest consumption of fuel; and although the cost of the apparatus is increased by the superior finish required for the radiators, which are here exposed to view, and by the greater length and complication of the mains, in comparison with the indirect method, yet the increase is about counterbalanced by the saving in extent of radiating surface and in diameter of mains with the direct system; and the special constructions of heating chambers, flues, and dampers, attending the indirect system, make the latter the more costly mode of warming any building.

The conditions of American ventilation and warming, in relation to health and comfort, differ materially from those prevailing for the same latitudes in Great Britain and other western parts of Europe. The temperature agreeable to Americans in cold weather[1] is about 70° Fahrenheit on the Atlantic coast, rising to 80° or 85° for inland localities and for the severest and driest cold. The American and European requirements differ therefore so completely, that in the Tables given in the appendix no attempt has been made to establish any relation between the area of radiating surface in the warming apparatus and the extent of building to be warmed, whether as regards floor area, outside surface of walls or roof, and cubic capacity, or in connection with ventilation for varying numbers of persons or of lights; the Tables are presumed to be equally applicable to data derived from experience on either side of the Atlantic.

In the buildings warmed in America by direct radiation there is generally no provision whatever of air-flues, either inlet or outlet, to aid ventilation. This is true for dwelling-rooms, offices, and mill or factory rooms. Recently in many of the better living-rooms, and in offices also, open fire-places have been built, mainly for show, but possessing some utility in producing a little radiating heat for speedy warming. Fire-places, while valuable for ventilation, are chiefly important from the advantages they offer for the attainment of an equable tempera-

---

[1] This branch of the question was touched upon by the Author in a Paper read before the American Institute of Architects at their Boston meeting in October 1877, and reprinted in January 1878 in the "Quarterly Journal of Science."

ture throughout a room. The outlet aperture in any room must be at or near the floor, at any rate in cold weather, so as to remove the air from the bottom of the room where it is coolest. Although the fouler air is of course at the top of every warm room, yet the discomfort consequent upon removing the warmer air from the top, and allowing the cooler air to stagnate below, is found too serious to be endured. A healthy atmosphere would indeed be maintained throughout the room, provided that, in removing air enough from the top, care were taken to supply an equal quantity of fresh air by a diffused admission suitably arranged for comfort; but complete ventilation will always fail to receive due consideration wherever it clashes with comfortable warmth. Gas-lights are used, to the exclusion of all others, in American dwellings; and no inconvenience or unhealthiness is considered to result from the unventilated burners. It is fully recognised that the vitiation of air by the combustion of gas is not organic; and although, when the products of combustion are in excess, the air becomes unbreathable, it is by no means the source of disease.

In warming a room by direct radiation, the proper situation of the radiating surface for the attainment of an equable temperature is in some respects a moot question. As long ago as 1846 Dr. Morrell Wyman[1] called attention to the way in which heat was distributed in any room, when the source of heat was situated as usual at the back of the room, against an inner wall, with windows and exposed outside wall at the front or side of the room; and he showed that a layer of highly heated air collected next the ceiling, whence descended a sheet of air along the cooler wall and window surfaces, becoming itself cooled in its descent, until at about the height of a man a uniform temperature of comfortable warmth was established throughout the room. It is in conformity with this principle that, in the practice of warming in America by means of a hot-air furnace, the hot-air flues from the furnace are generally led up inside an inner wall, remote from windows or outside walls, and a tolerably equable and comfortable warmth is obtained by admitting into the room a limited quantity of very hot air, sometimes nearly as hot as 400° at the inlet. A closed stove, situated likewise against an inner wall, proves similarly effective; but with an open fire the draught of cold air,

---

[1] Dr. Wyman published in Boston in 1846 "A practical treatise on Ventilation and Warming," containing much of novelty at the time and of lasting excellence. The work, now out of print, is as suggestive as Walker's "Hints on Ventilation" with which it may be compared.

which from some source or other takes its course along the floor to the fire-place, seriously impairs the desirable uniformity of temperature in a room.

In warming by steam with direct radiating surfaces, the practice for many years was to arrange the steam pipes in lines or groups, called coils, along the bottom of the outside walls or under the windows. But present usage seems to indicate that better results can be obtained by placing the radiating surface where it will apparently be warming what is already the hottest part of the room; in reality it then promotes the natural circulation, whereby the proper diffusion of the warmth is aided, instead of being retarded. For warming rooms in mills, the most recent practice is to place the direct radiating pipes in rows overhead, suspended a foot or two from the ceiling, and two or three feet from the outside walls. Although by this arrangement the heat would apparently be expended in the top of the room, yet very satisfactory results are thereby obtained, in regard both to equability of warming and to efficiency of radiating surface.

In rooms warmed by direct radiation, the attempt has repeatedly been made to effect the ventilation by admitting air direct upon the radiating surfaces; which are then placed under the windows, so as to intercept any descending currents of cooled air. Not much success has attended this plan, inasmuch as through some of the inlet apertures outward currents of warm air are then apt to escape from the room, producing no perceptible warming of the external air; while through others a flood of cold air enters from the outside, and, passing only some one coil or radiator, gets hardly warmed at all, the effect being far from comfortable.

Construction of Radiators.—For warming by direct radiation, the radiators usually consist of coils, composed of ¾-inch and 1-inch steam-pipes, which are arranged in parallel lines and are coupled to branch tees or heads. In a few exceptional cases, radiators of peculiar shapes are specially constructed. In all cases the coils must have either vertical or horizontal elbows of moderate length, for allowing each pipe to expand separately and freely. Sometimes short lengths of pipe are coupled by return-bends, doubling backwards and forwards in several replications one above another, and forming what are called "return-bend coils;" and when several of these sections are connected by branch tees into a compact mass of tubing, the whole is known as a "box-coil," Fig. 6.

As the amount of heat given off from the radiator cannot be satisfactorily controlled by throttling the steam-supply, it is usual to divide all radiators into sections, each of which can be shut off from the supply and return mains, separately from the rest of the sections. This method of regulation applies to radiators for indirect heating as well as for direct.

FIG. 6. RETURN-BEND OR BOX COIL.

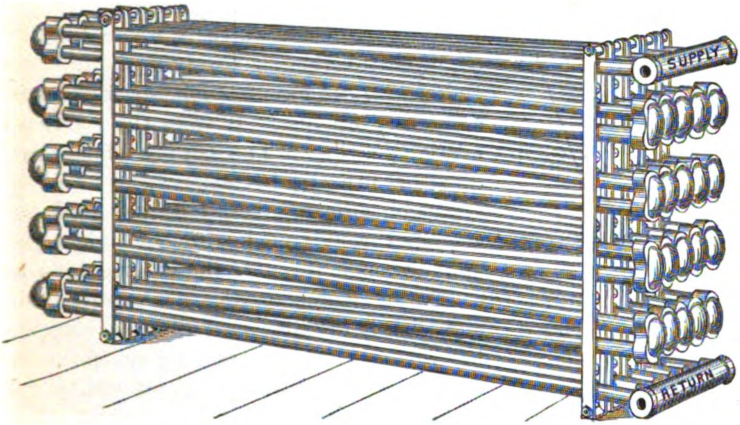
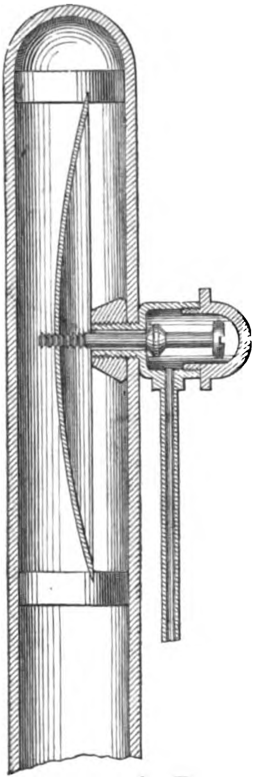


FIG. 7. VERTICAL-TUBE RADIATOR.

Vertical-pipe coils, Fig. 7, constitute a distinctive form of radiator now largely used. In these a number of short upright 1-inch tubes, from 2 feet 8 inches to 2 feet 10 inches long, are screwed into a hollow cast-iron base or box; and are either connected together in pairs by return-bends at their upper ends, or else each tube stands singly with its upper end closed, and having a hoop-iron partition extending up inside it from the bottom to nearly the top. The supply of steam is admitted into the bottom casting; and the steam on entering, being lighter than air, ascends through one leg of each siphon pipe and descends through the other, while the condensed water trickles down either leg, and with it the displaced air sinks also into the bottom box. For getting rid of the air, a trap is provided, having an outlet controlled by metallic rods; as soon as all the air has escaped and the rods become heated by the presence of unmixed steam, their expansion closes the outlet. In Fig. 8 is shown an arrangement adapted to a vertical radiator tube: a slightly curved strip of metal is placed in the top of the tube, with its ends abutting against fixed stops; when heated, the increased curvature resulting from its expansion closes the air-escape valve. The principle of both these traps is old. Besides the coils of vertical wrought-iron tubes, double pipes of cast iron are made to screw similarly into a bottom casting; and this construction competes in price with the use of wrought-iron tubes, for equal areas of radiating surface.

Fig. 8.



AUTOMATIC AIR-ESCAPE VALVE.

One construction of direct radiating surfaces that has had repeated trial consists of tablets of thin sheet-iron. These were proposed and used by James Watt, and were stayed by having their sides indented with recesses and quilted together. The cause of failure lies in the presence of air inside an iron vessel in which steam is condensing. Under no other condition of exposure does iron perish so rapidly. In the ordinary closed circulation, the wrought-iron tubes and cast-iron fittings are practically imperishable internally, owing to the entire exclusion of air. At any rate, during more than thirty years' experience the Author has never seen a pipe destroyed by rust by internal corrosion, although pipes rusted off from the outside are by no means uncommon.

For indirect radiating surfaces, the box coils are the forms most used. The chambers or casings for containing them are made either of brickwork, or often of galvanised sheet-iron of No. 26 gauge with folded joints. The coils are suspended freely within the chambers, which are themselves attached to the walls containing the air inlet flues. Besides coils of wrought-iron tubes, cast-iron tablets or hollow slabs, having vertical surfaces with projecting studs or ribs, have been extensively used for the radiating surfaces in warming houses with low-pressure steam. They are like the tablets common in England for warming with hot water. Their great advantage has been found to lie in their small height; as little as 6 to 8 inches is height enough to warm sufficiently the current of fresh air travelling only that distance in contact with their heating surface.

Ventilation combined with Warming.—Where systematic ventilation is carried out in conjunction with warming, the indirect radiators and chambers just described are employed. The regulation of the warmth to be supplied by the apparatus can be effected by dividing the coil into independent sections, one or more of which can be shut off at pleasure, as previously mentioned. But in combination with systematic ventilation the warming can also be more effectually controlled by so arranging the casing or chamber containing the coil, that the whole or any part of the fresh air entering can be made either to pass through the coil and be warmed, or to "by-pass" the coil and escape warming: the warmed and unwarmed currents are then mingled in a mixing chamber or flue, whence a supply of fresh air suitably tempered flows into the room. This method renders it feasible for the occupant of the room to control the tempering of the air, and thereby to regulate the warming of the room; it also ensures the constant supply of a definite quantity of air.

Where a blowing fan is employed as a means of impelling a current of air through a building, a further improvement for large and extensive warming apparatus is proposed, which consists in placing a large auxiliary warming coil at the entrance of the main supply-flue that leads from the fan into the building.[1] Provision is made for using either the whole of the auxiliary coil or only a

---

[1] This method is believed to have been first proposed by Major-General M. C. Meigs, U.S.A., when in charge of erecting the U. S. Capitol at Washington in 1856.

portion of it, and it is also provided with by-passages and regulating shutters or dampers: so that the air entering the main supply-flue to the building can at all times be warmed to a uniform temperature of say 50° Fahrenheit. The air thus warmed can be allowed to pass along flues wheresoever situated,—underground, through cellars, or under passages of a building,—without much loss of warmth, and without danger of injuring foundations by freezing. From these flues the air is admitted into the coil chambers previously described, where it undergoes the further warming requisite to give the desired temperature in the rooms. By this means, while the volume of fresh air entering a room remains constant, its warmth may be regulated to any temperature from 50° as a minimum up to 120° as a maximum. The extent of radiating surface distributed inside the rooms will by this arrangement be only about one-half of the total that has usually to be provided where no auxiliary warming coil is employed; while the large auxiliary coil itself has only about 40 per cent. of the total surface usually provided in its absence. Hence this arrangement actually saves about 10 per cent. of warming surface, irrespective of the saving in cost of steam mains when the boilers are placed near the fan or entrance to the main supply-flue.

The blowing fan generally employed in America for ventilating large buildings is that described by the Author in a former Paper to this Institution.[1] The exhaust steam from the engine driving the fan is utilised for warming some large coil; and any deficiency in quantity is made up by a supply of live steam, taken direct from the boiler through a "differential-pressure" valve, which is of common use where exhaust steam is employed for warming.

Examples of extensive Warming by Steam.—As an example of warming on an extensive scale may be taken a large office building in New York, of which the following are the particulars:—

Total number of rooms, including halls and vaults . .		286
Total area of floor surface	square feet	137,370
Total volume of rooms	. cubic feet	1,923,590
Number of constant occupants during office hours . .		650
Maximum average of occupants at any time		1,300
Volume per occupant, excluding vaults .	. cubic feet	1,443

In addition to the steam for warming, the boilers all furnish steam for the engine-power expended in working lifts or elevators in the building, in pumping the water-supply, and in electric lighting; and the same boilers also furnish steam for motive power and for warming to other buildings at several hundred feet

---

[1] Minutes of Proceedings Inst. C.E., vol. xxx., p. 276.

distance, this extra service absorbing about one-third of the total boiler capacity. The boilers are eight in number, and have altogether 173 square feet of grate area, with about 8,000 square feet of heating surface. The lifts or elevators convey about two million persons per year.

A second example is furnished by the State Lunatic Asylum at Indianapolis:—

Length of frontage of building, more than			2,000 lineal feet.
Total volume of rooms			2,574,084 cubic feet.
Warming apparatus	Indirect radiating surface	23,296	
	Direct	10,804	
	Total		34,100 square feet.
Boilers	Grate area		180 square feet.
	Heating surface		5,863 square feet.

This warming apparatus was constructed by the Walworth Manufacturing Co. of Boston, after the plans of their engineer, Mr. L. R. Greene, contrary to whose advice however the ventilation is effected by chimneys instead of by a blowing fan.

As examples of the extent of heat transmission from a central source may be mentioned the hospital at Columbus, Ohio, and that at Buffalo, New York, each warmed by steam, the former having a linear frontage of 2,280 feet, and the latter of about 3,000 feet.

While these dimensions give a notion of the magnitude of the warming apparatus for numerous large buildings, they fail to convey any idea of the very general prevalence of warming by steam in any of the commercial cities of America. The boilers in use for the purpose at any one warehouse are made to supply steam, for warming and for motive power, to any distance and to any extent within the limit of their capacity. The system is adopted in Boston and New York for the larger residences in flats or storeys, which are now rapidly coming into favour. There appears indeed no limit to the future extension of systematic steam-supply for warming and for motive power; and every facility is afforded for its growth, from the fact of the necessary mechanical details having already been fully worked out.

In the Appendix are enumerated the commonly accepted data which form the basis for computing the efficiency of the warming surfaces, the size of the mains, and the proportions of the various details; and tables are given of the formulas and figures most generally useful for working out the practical dimensions suited to any particular application.

The Paper is accompanied by several diagrams, from which the woodcuts have been prepared.

[Appendix.

APPENDIX.

For estimating the heat which will be given off from the radiating surface of a warming apparatus, and for calculating the flow of steam through tubes of various diameters, and for computing the resistance of inlets or of elbows, it is only needful to have recourse to data and formulas already available from the results of experimental research. While discrepancies among these results may preclude absolute accuracy, yet, where a limit of dimension is more essential than minute exactness, values generally accepted may safely be employed in the preparation of tables for general practical application.

The nine data accepted by the Author for the compilation of the Tables comprised in this appendix are the following.

1st. Each 100 square feet of radiating surface will give off 3 lb.-Fahrenheit units of heat per minute, for each degree Fahrenheit of difference in temperature between the radiating surface itself and the air in which it is exposed. This rate of radiation is limited to surfaces at a temperature of say from 180° to 300°, heated by steam or hot water, and employed for warming rooms; while the temperature of the air itself, during its contact with the warming surfaces, ranges within the limits of 20° and 100°. These conditions embrace the method described in the Paper of warming by direct radiation, where the temperature of the room, at the height of 4 to 5 feet above the floor, is to be maintained at an average of 65° to 70°; and also the method of indirect warming by currents of heated air, where the air may be supposed to have a temperature of only 20° on entering the warming chamber that contains the heating surfaces, and to issue thence at 100° into the room.

2nd. For convenience 60°, as the mean between 20° and 100°, will be taken as the average temperature of the air in which the radiating surface is exposed. That is to say, in cold weather the warmed air on quitting the radiators is required to be as many degrees above the mean temperature of 60° as the external cold air is below this mean on admission to the warming apparatus.

3rd. Regnault's data in regard to the properties of steam are accepted as authoritative.

4th. The flow of steam or water through a straight tube is given with sufficient accuracy by the approximate formula[1]

$$v = 50 \sqrt{\frac{h}{l} \times \text{inside diameter}}$$

where v is the velocity per second, h the corresponding head or height of column giving the effective pressure that causes the flow, and l the length of the tube: all these being expressed in feet, as is also the case

---

[1] For the coefficient 50 in this formula the following explanation has been kindly furnished by Prof. Unwin, by whom also the succeeding 5th and 6th data have been amended to their present form.

The frictional loss of head, when steam or water flows through a straight tube, is given by the equation $h = \zeta \frac{4\,l}{\text{diam.}} \frac{v^2}{2\,g}$; whence $v = \sqrt{\frac{64 \cdot 4}{4\,\zeta}\left(\frac{h}{l} \times \text{diam.}\right)}$ all the dimensions being in feet. According to Weisbach ζ has the value 0·00644; in which case $\sqrt{\frac{64 \cdot 4}{4\,\zeta}} = 50$. At the same time it is pointed out by Prof. Unwin that the value 0·00644 for the coefficient ζ is possibly too small for tubes of small bore: and he would put $\zeta = 0 \cdot 006$ to $0 \cdot 010$ for 4-inch tubes, and $\zeta = 0 \cdot 0084$ to $0 \cdot 012$ for 2-inch tubes: assuming steam to have the same coefficient as air, which is probably true enough.

here with the diameter. Then the quantity flowing in cubic feet per second $= v \times \frac{\pi}{4}(\text{diameter})^2 = 39{\cdot}27 \sqrt{\frac{h}{l} \times (\text{diameter})^5}$. But it is more convenient to take the quantity Q in cubic feet per minute, and the tube diameter d in inches, the length l and the head h remaining in feet; and when this has been done, the four variables will be given explicitly by the four following equations:—

$$Q = 4{\cdot}7233\sqrt{\frac{h\,d^5}{l}}; \qquad l = 22{\cdot}3096\,\frac{h\,d^5}{Q^2}; \qquad h = 0{\cdot}0448\,\frac{Q^2\,l}{d^5};$$

$$d = 0{\cdot}5374\sqrt[5]{\frac{Q^2\,l}{h}}$$

The last of these is here the important equation, since the internal diameter d of the tube is the dimension that is wanted to be determined for given values of Q, l, and h.

5th. The resistance at the entrance to a tube, when no special bell-mouth is used, consists of two parts. The head $\frac{v^2}{2g}$ is expended in giving the velocity of flow; and the head $0{\cdot}505\,\frac{v^2}{2g}$ in overcoming the resistance of the mouth of the tube. Hence the whole loss of head at the entrance of the tube is $1{\cdot}505\,\frac{v^2}{2g}$. This resistance is equal to the frictional resistance of a length l of straight tube given by the equation $\zeta\,\frac{4\,l}{\text{diam.}}\,\frac{v^2}{2g} = 1{\cdot}505\,\frac{v^2}{2g}$. Hence $l = \frac{1{\cdot}505}{4 \times 0{\cdot}00644} \times$ diameter $= 58{\cdot}4 \times$ diameter; that is to say, the entering current encounters as much resistance at the inlet of the tube as in subsequently flowing through a length of straight tube equal to about 60 times its diameter.

6th. From Weisbach's experiments on the resistance of elbows, it appears that the loss of head at each sharp right-angled elbow is $0{\cdot}989\,\frac{v^2}{2g}$, the radius of curvature for the centre line of the bend being equal to the radius of the bore. In the elbows actually used, the loss of head may be a little less, because of their being curved with rather a longer radius,[1] as seen in Fig. 3. Consequently the resistance at each elbow is equivalent to the head lost by friction in a length l of straight tube given by the equation $\zeta\,\frac{4\,l}{\text{diam.}}\,\frac{v^2}{2g} = 0{\cdot}989\,\frac{v^2}{2g}$. Whence $l = \frac{0{\cdot}989}{4 \times 0{\cdot}00644} \times$ diameter $= 38{\cdot}4 \times$ diameter; or the loss at each sharp right-angled elbow is the same as in flowing through a length of straight tube equal to about 40 times the tube diameter.

7th. For a "globe" steam stop-valve, in which the current of steam has to turn aside at right-angles to the line of the tube, in order to pass up through the seating of the valve and then back again into the line of the tube, the resistance is assumed at about half as much again as that of the sharp elbow dealt with in the preceding paragraph.

8th. It is accepted that, with the ordinary boiler, properly set, and with adequate draught and suitable proportions of heating surface, 1 lb. of anthracite coal of average quality will give out 9,000 units of heat to the steam generated.

9th. It is assumed that the consumption of coal per square foot of grate per hour is about 8 lbs. for small or domestic warming apparatus; 10 lbs. for larger

---

[1] Another reason why the calculation here given is no more than a rough approximation is that the diameter of the bore in the actual elbow or bend is about equal to the external, not to the internal, diameter of the tube.

apparatus requiring a special attendant; and 12 lbs. for very extensive **apparatus**, where regular firemen are employed, and a foreman to look after the **whole** o the warming.

TABLE V.

Physical properties of Steam and Condensed Water, under conditions of ordinary practice in warming by steam.

A	Steam Pressure per square inch { above atm. total . .	lbs. lbs.	0 14·7	3 17·7	10 24·7	30 44·7	60 74·7
B	Temperature of Steam . .	Fahr.	212°	222°	239°	274°	307°
C	Temperature of Air . . .	Fahr.	60°	60°	60°	60°	60°
D	Difference = B − C . . .	Fahr.	152°	162°	179°	214°	247°
E	Heat given out per minute per 100 sq. ft. of radiating surface = D × 3 . . .	units	456	486	537	642	741
F	Latent heat of steam . .	Fahr.	965°	958°	946°	921°	898°
G	Volume of 1 lb. weight of steam	cub. ft.	26·4	22·1	16·2	9·24	5·70
H	Weight of 1 cubic foot of steam	lb.	0·0380	0·0452	0·0618	0·1082	0·1752
J	Volume *Q* of steam per minute to give out E units = E × G ÷ F . . .	cub. ft.	12·48	11·21	9·20	6·44	4·70
K	Weight of 1 cubic foot of condensed water at temperature B	lbs.	59·64	59·51	59·05	58·07	57·03
L	Volume of condensed water to return to boiler per minute = J × H ÷ K	cub. ft.	0·0079	0·0085	0·0096	0·0120	0·0144
M	Head of steam equivalent to 12 inches water column = K ÷ H	feet	1569	1317	955·5	536·7	325·5
	STEAM-SUPPLY-MAINS.						
N	Head *h* of steam, equivalent to assumed 2 inches water column for producing steam flow *Q*, = M ÷ 6	feet	261·5	219·5	159·3	89·45	54·25
P	Internal Diameter *d* of tube* for flow *Q* when *l* = 1 foot	inch.	0·484	0·481	0·474	0·461	0·449
R	Do. do. when *l* = 100 feet	inch	1·217	1·207	1·190	1·158	1·128
S	Ratios of values of *d* . .	ratio	1·023	1·015	1·000	0·973	0·948
	WATER-RETURN-MAINS.						
T	Head *h* assumed at ½ inch water column for producing full-bore water flow *Q* . .	foot	0·0417	0·0417	0·0417	0·0417	0·0417
U	Internal Diameter *d* of tube* for flow *Q* when *l* = 1 foot	inch	0·147	0·151	0·158	0·173	0·186
V	Do. do. when *l* = 100 feet	inch	0·368	0·379	0·398	0·434	0·468
W	Ratios of values of *d* . .	ratio	0·926	0·952	1·000	1·092	1·176

* P, R, U, V, are each determined from the foregoing formula

$$d = 0{\cdot}5374 \sqrt[5]{\frac{Q^2\, l}{h}}.$$

Table V.—The foregoing data being accepted, the following Table V. is compiled as a preparatory step towards their practical application to apparatus for warming by steam. The chief physical properties that have to be dealt with, of the steam-supply and of the condensed water resulting therefrom, are here tabulated for the range of steam-pressure occurring in ordinary practice, namely from atmospheric pressure up to 60 lbs. per square inch above atmosphere.

The line of ratios S shows that, within the usual range of pressures in warming by steam, the diameters of the steam-supply-mains are practically the same for any given head of condensed water to produce the required flow; and the line of ratios W shows that the same is true for the water-return-mains. This conclusion is substantiated by practical experience in the working of the warming apparatus; but the two lines of ratios present the means of effecting the small correction needed for complete accuracy, were that desirable.

Table VI.—For readily computing the effect produced upon *d*, the internal diameter of tube, by variations in the values of the three several variables *Q*, *l*, and *h*, the foregoing expression for *d* is here repeated under the three following forms, in which each variable in turn is given explicitly.

$$d = \left(0{\cdot}5374\sqrt[5]{\frac{l}{h}}\right) \times Q^{\frac{2}{5}} = \mathrm{X} \times Q^{\frac{2}{5}} \quad . \quad . \quad . \quad . \quad . \quad . \qquad (1)$$

$$d = \left(0{\cdot}5374\sqrt[5]{\frac{Q^2}{h}}\right) \times l^{\frac{1}{5}} = \mathrm{Y} \times l^{\frac{1}{5}} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (2)$$

$$d = \left(0{\cdot}5374\sqrt{Q^2 l}\right) \times \frac{1}{h^{\frac{1}{5}}} = \mathrm{Z} \times \frac{1}{h^{\frac{1}{5}}} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (3)$$

And the following Table VI. gives the values of the function of each variable in turn, for different values of the variable itself:—

TABLE VI.

When $Q =$	1	2	3	4	5	6	7	8	9	10	12	14	16	18	20	30	32	(1)
$d = \mathrm{X} \times$	1·000	1·320	1·553	1·741	1·904	2·048	2·178	2·297	2·408	2·512	2·702	2·874	3·028	3·178	3·317	3·898	4·000	
When $l =$	1	2	3	4	5	6	7	8	9	10	12	14	16	18	20	30	32	(2)
$d = \mathrm{Y} \times$	1·000	1·149	1·246	1·319	1·380	1·431	1·476	1·515	1·552	1·585	1·640	1·695	1·741	1·782	1·820	1·974	2·000	
When $h =$	1	2	3	4	5	6	7	8	9	10	12	14	16	18	20	30	32	(3)
$d = \mathrm{Z} \times$	1·000	0·871	0·803	0·758	0·725	0·699	0·678	0·660	0·644	0·631	0·610	0·590	0·574	0·561	0·549	0·507	0·500	

Steam-Supply-Mains.—Tables VII. to X.—By using the figures of Table VI. in combination with those in lines P and R of Table V.,—noting that *Q* represents the cubic feet of steam required per minute for heating 100 square feet of radiating surface,—the internal diameter *d* of the steam-supply-mains could be calculated for all variations in extent of warming surface, in length *l* of main, and in head *h* producing the flow. In this general form however the application of the equations to immediate practice becomes tedious; and the trouble of recalling the method of applying them deters their use. They have been presented here for the purpose of substantiating the means whereby the following general Tables VII. to X. have been prepared: in which the proper internal diameter *d* for the steam-supply-mains is worked out through an extensive range of variations in the practical conditions of application. All four Tables are compiled for an assumed steam-pressure of 10 lbs. per square inch above atmosphere, the corresponding temperature of the steam being 239° Fahrenheit; and the ratios furnished in line 8 of the preceding Table V. are repeated at the foot of Tables VII. to X., as the factors by which the diameters given in these Tables have to be multiplied respectively for the several other pressures assumed in Table V. The total resistance to be overcome in the flow of the steam through the mains from the boilers to the radiators is assumed successively at 2, 6, 12, and 24 inches of water-column; and at foot of each Table are added the factors by which that Table may be converted into any of the other three: thus Table VII. is drawn up for a total resistance of 2 inches of water-column, and its diameters when multiplied by the factor 0·8027 become the diameters given in Table VIII. for a resistance of 6 inches head of water.

The diameters *d* given in Tables VII. to X. relate solely to steam-tubes which are straight, or, if curved, having a curvature of which the radius is very large indeed in comparison with the tube-diameter. Moreover in estimating the quantity Q of steam required for heating the radiators, the loss of heat in the steam-mains through which it flows has been neglected. In the employment of these tabular values therefore, two conditions of qualification have to be observed.

TABLE VII.

Internal Diameters of Steam-Supply-Mains, with total resistance equal to 2 inches of water-column.

Pressure of Steam, 10 lbs. per square inch above atm., Temperature 239° Fahr.

Formula, $d = 0\cdot5374 \sqrt[5]{\frac{Q^2 l}{h}}$; where d = internal diameter in inches;

$Q = 9\cdot2$ cubic feet of steam per minute per 100 square feet of radiating surface;
l = length of mains in feet; $h = 159\cdot3$ feet head of steam to produce flow.

Radiating Surface.	Internal Diameters in inches, for Lengths of mains from 1 foot to 600 feet.										
	1 foot.	10 feet.	20 feet.	40 feet.	60 feet.	80 feet.	100 feet.	200 feet.	300 feet.	400 feet.	600 feet.
Sq. Feet.	Inch.	Inch.	Inch.	Inch.	Inch.	Inch.	Inch.	Inch.	Inch.	Inch.	Inch.
1	0·075	0·119	0·136	0·157	0·170	0·180	0·189	0·216	0·234	0·248	0·270
10	0·19	0·30	0·34	0·39	0·43	0·45	0·47	0·54	0·59	0·62	0·68
20	0·25	0·39	0·45	0·52	0·56	0·60	0·62	0·72	0·78	0·82	0·89
40	0·33	0·52	0·60	0·69	0·74	0·79	0·82	0·95	1·03	1·09	1·18
60	0·39	0·61	0·71	0·81	0·87	0·93	0·97	1·11	1·21	1·28	1·39
80	0·43	0·68	0·79	0·90	0·98	1·04	1·09	1·25	1·35	1·43	1·55
100	0·47	0·75	0·86	0·99	1·07	1·14	1·19	1·36	1·48	1·57	1·70
200	0·62	0·99	1·14	1·30	1·41	1·50	1·57	1·80	1·95	2·07	2·24
300	0·73	1·16	1·34	1·53	1·66	1·76	1·84	2·12	2·30	2·43	2·64
400	0·82	1·30	1·50	1·72	1·86	1·98	2·07	2·37	2·57	2·73	2·96
500	0·90	1·43	1·64	1·88	2·04	2·16	2·26	2·60	2·81	2·98	3·23
600	0·97	1·53	1·76	2·03	2·20	2·33	2·43	2·79	3·03	3·21	3·48
800	1·09	1·72	1·98	2·27	2·46	2·61	2·73	3·13	3·40	3·60	3·90
1,000	1·19	1·88	2·16	2·48	2·69	2·85	2·98	3·43	3·71	3·94	4·27
1,200	1·28	2·04	2·33	2·67	2·90	3·07	3·21	3·68	4·00	4·23	4·59
1,400	1·36	2·15	2·47	2·84	3·08	3·26	3·41	3·92	4·25	4·50	4·88
1,600	1·43	2·27	2·61	3·00	3·25	3·44	3·60	4·13	4·49	4·75	5·15
1,800	1·50	2·38	2·74	3·14	3·41	3·61	3·78	4·34	4·70	4·98	5·40
2,000	1·57	2·48	2·85	3·28	3·55	3·76	3·93	4·52	4·90	5·19	5·63
3,000	1·84	2·92	3·36	3·85	4·18	4·43	4·63	5·32	5·77	6·11	6·63
4,000	2·07	3·28	3·76	4·32	4·69	4·96	5·19	5·96	6·47	6·85	7·44

For other resistances and pressures, multiply by the respective factors below:—

Water col.	6 ins.	12 ins.	24 ins.	Press. above at.	0 lbs.	3 lbs.	30 lbs.	60 lbs.
Mult. by	0·8027	0·6988	0·6084	Multiply by	1·023	1·015	0·973	0·948

TABLE VIII.

Internal Diameters of Steam-Supply-Mains, with total resistance equal to 6 inches of water-column.

Pressure of Steam, 10 lbs. per square inch above atm., Temperature 239° Fahr.

Formula, $d = 0\cdot5374 \sqrt[5]{\frac{Q^2 l}{h}}$; where d = internal diameter in inches; $Q = 9\cdot2$ cubic feet of steam per minute per 100 square feet of radiating surface; l = length of mains in feet; $h = 477\cdot8$ feet head of steam to produce flow.

Radiating Surface.	Internal Diameters in inches, for Lengths of mains from 1 foot to 600 feet.										
	1 foot.	10 feet.	20 feet.	40 feet.	60 feet.	80 feet.	100 feet.	200 feet.	300 feet.	400 feet.	600 feet.
Sq. Feet.	Inch.	Inch.	Inch.	Inch.	Inch.	Inch.	Inch.	Inch.	Inch.	Inch.	Inch.
1	0·060	0·095	0·109	0·125	0·136	0·144	0·151	0·175	0·189	0·200	0·217
10	0·15	0·24	0·27	0·31	0·35	0·36	0·38	0·43	0·47	0·50	0·55
20	0·20	0·31	0·36	0·42	0·45	0·48	0·50	0·58	0·63	0·66	0·77
40	0·27	0·42	0·48	0·55	0·59	0·63	0·66	0·76	0·82	0·87	0·95
60	0·31	0·49	0·57	0·65	0·71	0·75	0·78	0·81	0·97	1·03	1·11
80	0·35	0·54	0·63	0·73	0·79	0·83	0·87	1·00	1·08	1·15	1·24
100	0·38	0·60	0·69	0·79	0·86	0·88	0·95	1·09	1·19	1·26	1·36
200	0·50	0·79	0·90	1·04	1·13	1·20	1·26	1·44	1·56	1·67	1·80
300	0·59	0·93	1·07	1·23	1·33	1·41	1·47	1·77	1·85	1·96	2·12
400	0·66	1·04	1·20	1·38	1·49	1·60	1·68	1·91	2·07	2·20	2·38
500	0·72	1·14	1·31	1·51	1·64	1·74	1·82	2·09	2·26	2·40	2·60
600	0·78	1·22	1·41	1·63	1·77	1·88	1·96	2·24	2·43	2·58	2·80
800	0·87	1·38	1·59	1·83	2·00	2·10	2·20	2·51	2·73	2·89	3·13
1,000	0·95	1·51	1·74	2·00	2·16	2·29	2·40	2·76	2·98	3·16	3·43
1,200	1·03	1·62	1·88	2·15	2·33	2·47	2·58	2·96	3·21	3·40	3·68
1,400	1·09	1·72	1·99	2·28	2·47	2·62	2·74	3·15	3·41	3·61	3·92
1,600	1·15	1·81	2·17	2·41	2·61	2·76	2·89	3·31	3·60	3·81	4·14
1,800	1·20	1·90	2·20	2·52	2·74	2·90	3·04	3·48	3·77	4·00	4·34
2,000	1·26	1·98	2·29	2·64	2·85	3·02	3·16	3·83	3·93	4·17	4·53
3,000	1·47	2·34	2·60	3·09	3·35	3·56	3·72	4·28	4·64	4·91	5·33
4,000	1·67	2·62	3·02	3·47	3·96	4·08	4·17	4·79	5·20	5·50	5·98

For other resistances and pressures, multiply by the respective factors below:—

Water col.	2 ins.	12 ins.	24 ins.	Press. above at.	0 lbs.	3 lbs.	30 lbs.	60 lbs.
Mult. by	1·2457	0·8706	0·7579	Multiply by	1·023	1·015	0·973	0·948

Table IX.

Internal Diameters of Steam-Supply-Mains, with total resistance equal to 12 *inches of water-column.*

Pressure of Steam, 10 lbs. per square inch above atm., Temperature 239° Fahr.

Formula, $d = 0\cdot5374\sqrt[5]{\frac{Q^2 l}{h}}$; where d = internal diameter in inches;

$Q = 9\cdot2$ cubic feet of steam per minute per 100 square feet of radiating surface;
l = length of mains in feet; $h = 955\cdot5$ feet head of steam to produce flow.

Radiating Surface.	Internal Diameters in inches, for Lengths of mains from 1 foot to 600 feet.										
	1 foot.	10 feet.	20 feet.	40 feet.	60 feet.	80 feet.	100 feet.	200 feet.	300 feet.	400 feet.	600 feet.
Sq. Feet.	Inch.	Inch.	Inch.	Inch.	Inch.	Inch.	Inch.	Inch.	Inch.	Inch.	Inch.
1	0·052	0·083	0·096	0·131	0·119	0·126	0·132	0·151	0·164	0·174	0·189
10	0·13	0·21	0·24	0·27	0·30	0·32	0·33	0·38	0·41	0·43	0·48
20	0·16	0·27	0·32	0·36	0·39	0·42	0·43	0·50	0·55	0·57	0·61
40	0·23	0·36	0·42	0·48	0·52	0·55	0·57	0·67	0·72	0·76	0·82
60	0·27	0·43	0·52	0·57	0·61	0·64	0·68	0·78	0·85	0·90	0·97
80	0·30	0·48	0·55	0·63	0·69	0·73	0·78	0·88	0·95	1·00	1·09
100	0·33	0·52	0·60	0·69	0·75	0·80	0·83	0·95	1·04	1·10	1·19
200	0·43	0·69	0·80	0·91	0·99	1·05	1·10	1·26	1·37	1·45	1·57
300	0·51	0·83	0·94	1·06	1·16	1·23	1·29	1·48	1·61	1·70	1·85
400	0·57	0·91	1·03	1·20	1·30	1·39	1·45	1·66	1·80	1·91	2·07
500	0·63	1·00	1·15	1·32	1·42	1·51	1·58	1·82	1·97	2·09	2·26
600	0·68	1·07	1·23	1·42	1·54	1·63	1·70	1·95	2·12	2·25	2·44
800	0·76	1·20	1·39	1·59	1·72	1·83	1·91	2·19	2·38	2·52	2·73
1,000	0·83	1·32	1·51	1·74	1·88	2·00	2·09	2·40	2·60	2·76	2·99
1,200	0·90	1·43	1·63	1·87	2·03	2·15	2·25	2·58	2·80	2·96	3·21
1,400	0·95	1·51	1·73	1·99	2·16	2·28	2·39	2·74	2·96	3·15	3·42
1,600	1·00	1·60	1·83	2·10	2·28	2·41	2·52	2·89	3·14	3·33	3·61
1,800	1·05	1·67	1·92	2·20	2·39	2·53	2·65	3·04	3·29	3·49	3·78
2,000	1·10	1·74	2·00	2·30	2·49	2·63	2·75	3·16	3·43	3·63	3·94
3,000	1·29	2·04	2·35	2·70	2·93	3·10	3·24	3·72	4·04	4·28	4·64
4,000	1·45	2·30	2·63	3·02	3·28	3·47	3·63	4·17	4·53	4·80	5·20

For other resistances and pressures, multiply by the respective factors below:—

Water col.	2 ins.	6 ins.	24 ins.	Press. above at.	0 lbs.	3 lbs.	30 lbs.	60 lbs.
Mult. by	1·4310	1·1487	0·8706	Multiply by	1·023	1·015	0·973	0·948

TABLE X.

Internal Diameters of Steam-Supply-Mains, with total resistance equal to 24 inches of water-column.

Pressure of Steam, 10 lbs. per square inch above atm., Temperature 239° Fahr.

Formula, $d = 0 \cdot 5374 \sqrt[5]{\frac{Q^2 l}{h}}$; where d = internal diameter in inches;

$Q = 9 \cdot 2$ cubic feet of steam per minute per 100 square feet of radiating surface;
l = length of mains in feet; $h = 1911$ feet head of steam to produce flow.

Radiating Surface.	Internal Diameters in inches, for Lengths of mains from 1 foot to 600 feet.										
	1 foot.	10 feet.	20 feet.	40 feet.	60 feet.	80 feet.	100 feet.	200 feet.	300 feet.	400 feet.	600 feet.
Sq. Feet.	Inch.	Inch.	Inch.	Inch.	Inch.	Inch.	Inch	Inch.	Inch.	Inch.	Inch.
1	0·046	0·072	0·083	0·096	0·104	0·110	0·115	0·132	0·143	0·151	0·164
10	0·116	0·183	0·21	0·24	0·26	0·27	0·29	0·33	0·36	0·38	0·42
20	0·15	0·24	0·28	0·32	0·34	0·37	0·38	0·54	0·47	0·50	0·54
40	0·20	0·32	0·37	0·42	0·45	0·48	0·50	0·58	0·63	0·67	0·72
60	0·24	0·37	0·43	0·49	0·53	0·57	0·59	0·68	0·72	0·78	0·85
80	0·26	0·42	0·48	0·55	0·60	0·63	0·67	0·76	0·82	0·87	0·95
100	0·29	0·46	0·53	0·60	0·65	0·70	0·73	0·83	0·90	0·96	1·04
200	0·38	0·60	0·69	0·79	0·86	0·92	0·96	1·10	1·19	1·26	1·36
300	0·45	0·71	0·82	0·93	1·02	1·07	1·14	1·29	1·40	1·48	1·61
400	0·50	0·79	0·92	1·05	1·14	1·21	1·26	1·44	1·57	1·67	1·81
500	0·55	0·87	1·00	1·15	1·24	1·32	1·38	1·59	1·72	1·82	1·97
600	0·59	0·93	1·07	1·24	1·34	1·44	1·48	1·70	1·85	1·96	2·12
800	0·67	1·04	1·21	1·38	1·50	1·60	1·67	1·91	2·07	2·20	2·38
1,000	0·73	1·15	1·32	1·51	1·64	1·74	1·82	2·09	2·27	2·40	2·60
1,200	0·78	1·24	1·42	1·63	1·77	1·87	1·96	2·25	2·44	2·58	2·80
1,400	0·83	1·31	1·50	1·73	1·88	1·99	2·08	2·39	2·59	2·74	2·98
1,600	0·87	1·38	1·60	1·83	1·98	2·09	2·20	2·52	2·74	2·90	3·14
1,800	0·92	1·45	1·68	1·91	2·07	2·21	2·31	2·64	2·87	3·04	3·29
2,000	0·96	1·51	1·74	2·00	2·16	2·30	2·40	2·76	2·99	3·17	3·44
3,000	1·12	1·78	2·05	2·35	2·55	2·70	2·83	3·24	3·52	3·73	4·05
4,000	1·26	2·00	2·30	2·63	2·86	3·03	3·16	3·64	3·94	4·18	4·53

For other resistances and pressures, multiply by the respective factors below:—

Water col.	2 ins.	6 ins.	12 ins.	Press. above at.	0 lbs.	3 lbs.	30 lbs.	60 lbs.
Mult. by	1·6438	1·3195	1·1487	Multiply by	1·023	1·015	0·973	0·948

Table XI.—The first qualification is that the resistances encountered by the steam in its flow through the inlet orifices of the tubes, through elbows, and through stop-valves, have to be estimated by their equivalent lengths of straight tube; and the sum of these lengths being then deducted from the tabular length *l* of straight tube, whereby the internal diameter has been arrived at, the remainder will be the effective length for which that diameter holds good. The corrections of length, needful to compensate for these several resistances to the flow of the steam, are given in Table XI.

The second qualification affecting Tables VII. to X. relates to the loss of heat from the surface of the steam-supply-mains, and involves a virtual reduction in the area of radiating surface that can be heated. The correction will be made by adopting the effective length of mains, as diminished by the preceding correction, and computing from Table II. their corresponding area of external surface: the radiating value of which, when the mains are properly clothed, may be accepted as averaging in practice for all situations about one-third of that of an equal area in the radiators themselves. The radiating value of the return-water-mains may be accepted as half that of the steam-mains, or one-sixth that of an equal area of surface in the radiators.

Table XII.—The effect of these two corrections is illustrated by the twenty-eight examples given in Table XII., in which the corrections are worked out roughly for cases falling within the range of Table X. The concluding column L of Table XII. represents approximately the ratio of the cost of the mains—both supply and return together—to that of the plain unornamented surface of the radiators. Moreover the percentages given in this column, when divided by 6, are approximately the ratios of the loss of heat from the whole of the supply- and return-mains, at the time when the maximum warming is being produced by the radiators. The divisor 6 results from the foregoing assumption that the loss per square foot from the surface of the steam-supply-mains is one-third of the heat given out from the radiators per square foot, and the loss from the water-return-mains one-sixth; whence the ratio of loss from the supply- and return-mains together is half the ratio of column H to column K, or one-sixth of the percentages L. The average loss however should be taken at double as much, in order to allow for the times when the full warming effect is not wanted from the radiators; and the percentages L have therefore to be divided by 3 for the average loss of heat, instead of by 6. It will be seen that the loss as here estimated ranges from one-hundredth to upwards of thirteen times the entire heat given out by the radiators; while the cost of the mains ranges from one-thirtieth up to forty times that of the radiators in the extreme examples.

TABLE XI.

Resistance to Flow through Inlet-Orifices, Elbows, and "Globe" Stop-Valves; measured by length of straight tube presenting equal resistance.

Inside Diameter of Tube.		RESISTANCE TO FLOW.		
Nominal.	Actual. *d*.	Each Inlet. *d* × 60.	Each Elbow. *d* × 40.	Each Valve.[1] *d* × 60.
Inches.	Inches.	Feet.	Feet.	Feet.
½	0·623	3·1	2·1	3·1
¾	0·824	4·1	2·7	4·1
1	1·048	5·2	3·5	5·2
1¼	1·380	6·9	4·6	6·9
1½	1·611	8·0	5·4	8·0
2	2·067	10·3	6·9	10·3
2½	2·468	12·3	8·2	12·3
3	3·067	15·3	10·2	15·3
3½	3·548	17·7	11·8	17·7
4	4·026	20·1	13·4	20·1
4½	4·508	22·5	15·0	22·5
5	5·045	25·2	16·8	25·2
6	6·065	30·3	20·2	30·3
7	7·023	35·1	23·4	35·1
8	7·982	39·9	26·6	39·9
9	9·001	45·0	30·0	45·0
10	10·019	50·0	33·3	50·0
12	12·000	60·0	40·0	60·0

---

[1] The resistance of each "globe" stop-valve is assumed at half as much again as that of a sharp elbow.

TABLE XII.

Reduction in Area of Radiating Surface, to compensate for loss of heat from surface of steam-supply-mains.

A Assumed Length *l* of steam main.	B Assumed Area of Radiating Surface.	C Corresponding Inside Diameter of steam main (Table X).	D Nearest Nominal Inside Diameter of wrought-iron Tubes (Table II).	E[1] Assumed Number of Elbows.	F Length of main equivalent to E elbows and one inlet and one valve (Table XI).	G Corrected Length (= A − F) for main of nominal size D.	H Area of outside surface of main for length G and size D (Table II).	J Radiating Surface equivalent to H (= H ÷ 3) when main is properly clothed.	K Corrected Area of Radiating Surface (= B − J).	L[2] Joint Cost of Supply and Return Mains, in percentage of Cost of Radiators (= 300 × H ÷ K).
Feet.	Sq. Ft.	Inch.	Inch.	No.	Feet.	Feet.	Sq. Ft.	Sq. Ft.	Sq. Ft.	Per cent.
40	60	0·49	½	4	15	25	6	2	58	31
40	100	0·60	½	4	15	25	6	2	98	18
40	200	0·79	¾	3	16	24	7	2	198	11
40	500	1·15	1¼	1	18	22	10	3	497	6
40	1,000	1·51	1½	1	22	18	9	3	997	3
40	2,000	2·00	2							
40	4,000	2·63	3							
100	60	0·59	½	13	34	66	15	5	55	82
100	100	0·73	¾	8	30	70	19	6	94	61
100	200	0·96	1	6	31	69	24	8	192	37
100	500	1·38	1¼	4	32	68	30	10	490	18
100	1,000	1·82	2	2	34	66	41	14	986	12
100	2,000	2·40	2½	2	41	59	45	15	1985	7
100	4,000	3·16	3½	1	47	53	56	19	3981	4
200	60	0·68	¾	18	57	143	39	13	47	249
200	100	0·83	¾	18	57	143	39	13	87	134
200	200	1·10	1¼	10	60	140	61	20	180	102
200	500	1·59	1½	8	59	141	70	23	477	44
200	1,000	2·09	2	6	62	138	86	29	971	27
200	2,000	2·76	3	4	71	129	118	39	1961	18
200	4,000	3·64	4	2	67	133	157	52	3948	12
600	60	0·85	¾	20	62	538	148	49	11	4036
600	100	1·04	1	20	80	520	179	60	40	1343
600	200	1·36	1¼	20	106	494	215	72	128	504
600	500	1·97	2	20	159	441	274	91	409	201
600	1,000	2·60	3	14	173	427	391	130	870	135
600	2,000	3·44	3½	12	177	423	443	148	1852	72
600	4,000	4·53	4½	9	180	420	549	183	3817	43

---

[1] **E.** The elbows are assumed to occur at intervals of not less than 120 times the nominal inside diameter **D** in the corrected length of main **G**, and never to exceed twenty in number.

[2] **L.** The outside surface of the water-return-mains, which are of the same length as the steam-supply-mains, is assumed to be only half that of the steam mains; and the cost per square foot of either surface is taken at double the cost per square foot of the radiating surface.

Water-Return-Mains.—The diameters to be given to the mains for the return of the condensed water are indicated in lines **U**, **V**, and **W** of Table V., on the assumption of ½ inch head of water-column for overcoming the total resistance encountered by the return-current. If the head were reduced to even as little as only ¼ inch of water-column, the sole difference would be that the figures in lines **U** and **V** would have to be multiplied by the factor 1·1487. Whence it is evident that the return-mains could in all cases be made very small, were it only the condensed water that had to run through them. In reality however the return-water-mains are encumbered with air, which in greater or less quantities always accompanies the steam, and requires room to travel forwards with the return-current of condensed water. This has led to the establishment of the practical rule that the diameter of the water-return-mains shall in general be about half that of the steam-supply-mains; and this assumption is adopted in column **L** of Table XII. and the corresponding foot-note.

TABLE XIII.

Quantity of Air warmed by radiators; Consumption of Coal; Area of Fire-Grate and of Boiler-Heating-Surface: all estimated per 100 *square feet of warming surface in radiators.*

A	Steam-pressure per square inch above atmosphere .	lbs.	0	3	10	30	60
B	Heat from radiators per minute (Table V.) . .	units	456	486	537	642	741
C	Volume of Air per minute warmed 1° Fahr. = **B** ÷ (0·0764 × 0·2377)	cub. ft.	25,110	26,762	29,570	35,352	40,803
D	Ratios of efficiency of radiators	ratio	**1·000**	1·066	1·178	1·408	1·625
		”	0·938	**1·000**	1·105	1·321	1·525
		”	0·849	0·905	**1·000**	1·196	1·380
		”	0·710	0·757	0·836	**1·000**	1·154
		”	0·615	0·656	0·725	0·866	**1·000**
E	Coal burnt per hour = **B** × 60 ÷ 9,000 . . .	lbs.	3·04	3·24	3·58	4·28	4·94
	FIRE-GRATE AREA.						
F	Small apparatus burning per sq. foot per hour, 8 lbs.	sq. ft.	0·380	0·405	0·448	..	..
G	Medium apparatus 10 lbs.	”	..	0·324	0·358	0·428	..
H	Large apparatus 12 lbs.	”	..	..	0·298	0·357	0·412
	BOILER-HEATING-SURFACE.						
J	Per 100 square feet of radiators = **E** × 2·8 . .	sq. ft.	8·512	9·072	10·02	11·98	13·83
K	Per square foot of fire-grate burning per hour 8 lbs. = 2·8 × 8	”	22·4	22·4	22·4	..	..
L	Do. 10 lbs. = 2·8 × 10	”	..	28·0	28·0	28·0	..
M	Do. 12 lbs. = 2·8 × 12	”	..	..	33·6	33·6	33·6

Table XIII.—In the preparatory Table V. the efficiency of the warming apparatus has been represented in terms of the units of heat given out per minute per hundred square feet of radiating surface. Another desirable form in which to represent it is the volume of air that can be warmed; and Table XIII. accordingly shows at C the cubic feet of air per minute which would be raised 1° Fahrenheit in temperature by the heat given out from each hundred square feet of radiating surface, assuming the specific heat of air at constant pressure to be 0·2377, and the weight of 1 cubic foot of dry air at atmospheric pressure and at 60° Fahrenheit to be 0·0764 lb. For the use of line C in this Table, any correction for moisture existing in the air may be neglected; and agreeably with the second datum it is assumed that the range of temperature through which the air is warmed extends always equally above and below the constant mean temperature of 60° Fahrenheit, winter requiring the warmed air to be as many degrees above 60° as the external cold air is below. The ratios of efficiency of the radiators for the several steam-pressures assumed are given at D. The consumption of coal per hour to produce the requisite warming is shown at E, on the assumption that 9,000 units of heat are utilised in raising steam per pound of anthracite coal burnt. The consequent fire-grate area requisite is given at F, G, and H, for the three rates of combustion already specified in the ninth datum: namely 8 lbs., 10 lbs., and 12 lbs. of coal per square foot of grate per hour. Line J gives the extent of heating surface required in plain tubular boilers, on the assumption that 2·8 square feet are needed per pound of anthracite coal burnt per hour; and lines K, L, and M, give the corresponding ratios of boiler-heating-surface to fire-grate area for the three several rates of combustion.

Areas of Radiators.—Table XIV.—Conversely to the tables of diameters for the steam-supply-mains, Table XIV. gives the areas of radiating surfaces that can be warmed by mains of various diameters and lengths. The formula to employ in this case is $Q = 4 \cdot 7233 \sqrt{\frac{h\,d^5}{l}}$; and assuming the steam-pressure at 10 lbs. per square inch above atmosphere, Q is seen from line J of Table V. to be 9·2 cubic feet of steam per minute per 100 square feet of radiating surface. Whence Area warmed $= Q \div 0 \cdot 092 = 51 \cdot 34 \sqrt{\frac{h\,d^5}{l}}$. The results of this formula are exemplified in Table XIV. for the standard sizes of wrought-iron tubes, and for the four lengths l of mains already dealt with in Table XII., the total resistance of which is here assumed as equivalent in each case to 24 inches of water-column or 1911 feet head h of steam. For the other resistances and pressures previously assumed, the areas here given require to be multiplied by the respective factors appended at foot of Table XIV. For change of steam-pressure, these factors are arrived at by combining the reciprocals of the ratios deduced from line J of Table V. with the square roots of the ratios deduced from line M of the same Table, so as to allow for the higher or lower warming power of steam at a higher or lower pressure than that assumed in Table XIV. Thus in order to ascertain from the present Table the larger area of radiators which the same mains could supply if the higher pressure of 60 lbs. per square inch above atmosphere were substituted, the factor to multiply by will be $\frac{9 \cdot 20}{4 \cdot 70} \times \sqrt{\frac{325 \cdot 5}{955 \cdot 5}} = 1 \cdot 142$. For application in practice, the areas in Table XIV. are of course subject to the corrections indicated by the foregoing Tables XI. and XII.

TABLE XIV.

Areas of Radiating Surfaces Warmed by steam-supply-mains of various diameters and lengths with total resistance equal to 24 inches of water-column.

Pressure of Steam, 10 lbs. per square inch above atm., Temperature 239° Fahr.

Area warmed $= 51 \cdot 34 \sqrt{\frac{h\,d^5}{l}}$; where d = internal diameter in inches; l = length of mains in feet; $h = 1911$ feet head of steam to produce flow.

Inside Diameters of steam-mains.		Areas in square feet, for Lengths l in feet.			
Nominal.	Actual $= d$.	$l = 40$ feet.	$l = 100$ feet.	$l = 200$ feet.	$l = 600$ feet.
Inches.	Inches.	Square Feet.	Square Feet.	Square Feet.	Square Feet.
⅛	0·270	13	9	6	3
¼	0·364	28	18	13	7
⅜	0·494	61	38	27	16
½	0·623	109	69	49	28
¾	0·824	219	138	98	56
1	1·048	399	252	178	103
1¼	1·380	794	502	355	205
1½	1·611	1,169	739	523	302
2	2·067	2,177	1,377	974	562
2½	2·468	3,396	2,148	1,519	877
3	3·067	5,846	3,697	2,614	1,509
3½	3·548	8,414	5,322	3,763	2,173
4	4·026	11,541	7,299	5,161	2,980
4½	4·508	15,311	9,684	6,848	3,953
5	5·045	20,287	12,830	9,072	5,238
6	6·065	32,146	20,331	14,376	8,300
7	7·023	46,383	29,335	20,743	11,976
8	7·982	63,876	40,399	28,566	16,493
9	9·001	86,255	54,552	38,574	22,271
10	10·019	112,750	71,310	50,423	29,112

For other resistances and pressures, multiply by the respective factors below:—

Water col.	2 ins.	6 ins.	12 ins.	Press. above at.	0 lbs.	3 lbs.	30 lbs.	60 lbs.
Mult. by	0·2887	0·5000	0·7071	Multiply by	0·945	0·964	1·071	1·142

Discussion.

Sir F. Bramwell, Vice-President, remarked that as the Paper was a posthumous one, it would be impossible to return a vote of thanks to the Author. All the Members could do was to express their appreciation of the Paper, and the hope that it might be followed by a useful discussion. Sir Frederick Bramwell.

Captain Douglas Galton was extremely sorry to hear the Author of the Paper referred to as "the late Robert Briggs," not being aware that he had died. The Paper itself was very interesting, as furnishing many of the details of the system which the Author had had the opportunity of elaborating during many years in the United States. The system of steam-heating had taken root in that country to a large extent. The climate was so cold in winter that it seemed as if that method of heating were more advantageous there than it had been apparently found in this country. Steam-pipes heated from 220° to 300° were more advantageous for warming the air than hot-water pipes, whose heat varied from say 120° to 180°, and therefore smaller pipes might be used. He regretted that the Author had not given some estimate of the amount of coal consumed by heating by steam as compared with that consumed by heating by hot water by some of the systems in use in England; for probably the use of steam would be found more expensive in fuel, although it might be more economical in first cost. But, of course, it was very difficult to compare the heating in one country with that in another, because the conditions under which the heating was carried on were so very various. In England the climate was damp and mild; while in America it was dry and cold. In England it was not required to communicate heat to the houses to the same extent as in America; but no one who had spent a winter in the United States, and in those towns where steam-heating was in use, could have failed to feel that the system as practised there left much to be desired—not in the extent of communicating heat to the houses, but with reference to the comfort of individuals. The heat given out by the radiators, or coils of pipes, was so intense, and it was so seldom that any arrangement was made to effect proper ventilation in the rooms where the radiators were placed, that a sense of oppression was generally felt in the rooms heated by steam, that he had himself visited in the United States. He did not think that result was a necessary consequence of the use of steam in heating; it arose from insufficient attention being paid to the admission of fresh air and the removal of foul air in con- Capt. Douglas Galton.

Capt. Douglas Galton. nection with the warming. But it certainly was the case that steam-heating as applied in the United States was very oppressive and uncomfortable to an Englishman. He thought the value of the Author's Paper lay in the details which he had given of the construction of the various joints and other appliances for carrying on the system of heating. Steam heating had been long known and practised in this country. The Author had mentioned several large buildings in New York, but the Houses of Parliament in London were heated by steam on a much larger scale than anything which had been mentioned. Captain Galton therefore thought that there was not much to learn from the United States in that matter.

Mr. Phipson. Mr. W. W. Phipson observed that the application of steam to the warming of buildings had been well known in England for many years. The Author of the Paper must have forgotten the valuable treatise on Steam-Heating of Thomas Tredgold, Hon. M. Inst. C.E., three editions of which had been published in this country between 1820 and 1836, when he asserted that the only book published in the English language was Robertson Buchanan's treatise (1811 to 1814). In fact he thought Buchanan's and Tredgold's researches and experiments on the measures and effects of fuels, and their experiments on cooling with the practical data derived therefrom, had formed the basis of nearly all modern improvements in steam-heating. Several important buildings had been heated in this country on the principle of the closed circulation of steam by Messrs. Bailey, of Holborn, and others, between 1811 and 1821; and in one of these applications Tredgold stated that the steam-boilers were 550 feet distant from some of the points warmed, with only 2° of difference indicated by the thermometer when placed against the pipes near the boilers and against the pipes at 550 feet from the boilers. However, with all the then well-known advantages of steam-heating, it had given way to the principle of the hot-water circulation-apparatus, it having been proved that the latter was more simple in action, much safer, and at the same time meeting all the requirements of the English climate. But he was of opinion that the time was not far distant when Englishmen would follow the example of the American and French engineers, and that the heating of public and private institutions by the existing systems of hot-water circulation on the low or high pressure would be superseded by steam apparatus of low-pressure circulation, when the advantages and improvements of late brought to bear on these applications were more thoroughly known. He noticed that in the American practice of fitting up

Mr. Phipson.

steam-apparatus the use of cast-iron fittings was much advocated by the Author of the Paper. The perfection now attained in the manufacture of wrought-iron fittings made it an open question whether (except in special cases) any advantages would be gained by the adoption, in this country, of cast-iron fittings. The method of connecting tubes with taper threads and sockets was a universal practice, but Mr. Phipson had often seen in France back nuts employed, in addition to the sockets, to ensure a still better joint. The Seguin tubular boiler described by the Author as that most used by the Americans for an extended apparatus, was very similar to the type of boiler adopted by French engineers in steam-heating applications. Personally he was not much in favour of tubular boilers, and where space was no object a boiler of more simple construction, with plenty of steam room, was certainly to be preferred, and presented every facility for cleaning out. He could have wished the Author had expressed an opinion as to what was the best principle of apparatus, whether the closed or the open circulation. The Americans, he believed, were rather in favour of a closed circulation, or gravity apparatus, provided they could get as a minimum 4 feet from the lowest point of their distributing mains to the water-line of the boiler. The French, however, adopted open circulation in all the applications he had seen, preferring to return the condensed water to the boiler by mechanical means. The gravity-apparatus needed more care in erection, and was often troublesome in clearing the air and condensed water, so as to ensure regular action. When, however, the level would allow, it was to be preferred, on account of its greater economy, and with pipes of sufficient diameter it could be run at any desired pressure. The Author's remarks that the distribution of steam through the warming pipes was effected in an almost unlimited variety of ways, each possessing advantages for special cases, showed what great elasticity application of steam possessed. Without following all the variety of distribution given in the Paper, he was convinced that any that hindered the free return of the condensed water and the escape of air would always be more or less noisy in working. Tredgold invariably advocated taking the steam directly to its highest point of distribution and then allowing gradually to descend, bringing with it the condensed water and the air. From the observations he had made on high-level distribution, and the apparatus he had executed on this principle, Mr. Phipson found that it always worked well and noiselessly, with an even temperature on each floor. There were cases in which the high-level distribution could not be adopted, and

Mr. Phipson. then modifications, more or less complicated, had to be resorted to. The French practice had been various as regards these points. High-level distribution, the successive distribution by floors, with separate risers and return risers had been tried, and, finally, low-level distribution; but in all cases the steam flowed to the top of the heating surfaces, thus maintaining in an altered form the downward distribution. In the various applications which he had lately inspected in France the apparatus worked noiselessly. With these systems for the distribution of steam through the heating pipes it was difficult to draw a general conclusion as to which was the best in principle. In case the high-level or downward distribution was carried out, pipes of smaller diameter might be used for the mains than when the low-level distribution was adopted; but in all cases the disposition and levels of the mains depended much on the class of building to be heated. He agreed with the Author that direct radiation was preferable to any other means of heating rooms, and even in a systematic apparatus for warming and ventilation, as much of the heating surface as could be employed as a direct radiator was certain to prove a more successful application than if all the power of the apparatus were devoted to indirect radiation. He preferred placing radiators in a steam apparatus under the windows or against the coldest wall, and in no case using more than single-pipe radiators with heating surfaces of from 20 to 50 square feet. The French engineers did not adopt radiators on the American principle, but coils of pipes with a positive circulation, close vessels, and spaced columns, with a heating surface varying between 25 to 40 square feet. They also used a ventilating steam column resembling to some extent a Post-Office pillar-box, in which the condensed water was utilized in conjunction with the steam for warming. This was done by the steam-supply pipe being taken up inside the column to within 1 foot to 1 foot 6 inches of the top; the outlet of the condensed-water pipe being also kept at that level. The steam in condensing filled the vessel, and when the level of the resulting water reached the mouth of the condense pipe it flowed out through a small trap into the main return-pipe. These columns were often set in entrance halls, staircases, and main corridors, the object being to retain the heat longer in these spaces, after the steam had been shut off, by utilizing the heat retained in the condensed water. The Author had been at great pains in preparing this interesting account of the American practice of warming by steam, and if the data upon which the Tables in the appendix were based might be accepted, he had supplied a valuable addition to the knowledge possessed on this important subject. Mr. Phipson.

Mr. W. Anderson said he need hardly add anything to what had been stated, as to the value of the Paper as an important addition to the Minutes of Proceedings of the Institution, especially in the tabular matter. He wished, however, to take exception to the method of joining pipes upon the description of which the Author had bestowed so much space. He had admitted that there were two defects in that method: one that the screws had to be cut very accurately, and the other that a jointing material of some sort had to be smeared over the screws in order to ensure their being tight. He believed that Perkins' plan of making a joint was much better. The ends of the pipes, on Perkins' plan, were screwed with an ordinary right- and left-handed thread; a right- and left-handed coupling was adopted, and no particular care was necessary with the threads. The end of one pipe was flat, and the end of the other was V-shaped. Simply by turning the coupling the V-ended pipe was pressed into the flat-ended pipe, and the joint was made without any jointing material whatever, except that occasionally a soft copper ring was inserted between the ends of the pipes. Joints of that character were used for the very highest pressure in hydraulic- and steam-piping. A few years ago he had to lay about 9 miles of 3-inch piping in the Caucasus for conveying petroleum from Baku to the Caspian Sea, and it became necessary to ascertain what was the most practicable way of making the joints. He had heard of the taper-joint, and tried it against the joint which he had just described, but he found it almost impossible to make a tight joint for petroleum, because there was scarcely any jointing material that would not be dissolved by it, and without such material it would not be absolutely tight. The joint he had described with a copper washer between the ends was made tight without any trouble, and 9 miles of piping were laid by common Tartar labourers, superintended by only one Englishman, and in that entire length he did not think that there were half-a-dozen bad joints. No great accuracy was required—nothing but the simplest manipulation. With regard to the most suitable boilers for warming apparatus, he thought a great deal depended upon the sort of building that had to be warmed. In the case of a private house, a greenhouse, or a small building, the boiler should be set in brickwork, and have a large furnace, because it should not be fired more than twice a day. A large mass of fuel was needed, and the dampers should be kept nearly closed. Except in very cold weather, combustion went on slowly, and but little Mr. Anderson.

Mr. Anderson. attention was required. Even when the fuel was burnt out the brickwork would continue to heat the water and to keep up the circulation for a considerable time. In large buildings, where there were attendants on the spot day and night, of course a boiler of the most advantageous kind for getting the best duty out of the coal could be used. In the Tables compiled by the Author it had been assumed that the emission of heat was at the rate of 3 Fahrenheit units per square foot per difference of degree per hour. It appeared to him that the Author was either unaware of the experiments of Dulong, and Petit, and Hopkins' formula, or that he did not think it necessary to allude to them; he rather doubted the latter proposition because the Author had gone into his calculations with so much precision. The emission of heat from heating surfaces was not directly as the difference of temperature, and it depended besides upon the absolute temperature of the atmosphere in which the apparatus stood. About five years ago he made some experiments with a view of determining the coefficients of Hopkins' formula for pipes and coils as actually used in practice, and he constructed two curves, one for 4-inch and another for 2-inch pipes, cast iron and wrought iron, giving the rate of emission at various degrees of temperature at a standard temperature of atmosphere of 60°.[1] The experiments were made to determine the coefficient m in Hopkins' formula, and the result was that its mean value for the 3-inch cast-iron pipes in the coil was 132. The value of m for 2-inch pipe was 250, and for a 4-inch pipe 122. These experiments demonstrated that the rate of emission for differences of temperature of 200° and 100° differed by as much as 22 per cent. At 200° there were 22 per cent. more heat given out per square foot per difference of degree per hour than at 100°. Consequently the Author's Tables, made upon the basis of the uniform emission of heat for each degree of difference of temperature were erroneous. With regard to the relative advantages of steam and water, that matter depended upon the nature of the building to be warmed. Where there was complete control of the apparatus, for instance, when the fire could be regulated at pleasure, as in the case of a private house, or a greenhouse, no doubt water was the best means of heating; because from its very high specific heat, and the large quantity of water contained in the pipes, there was a reservoir of heat which, when the fire went out, would continue to keep up the warmth. Where, however, there was no such control of the heating apparatus, as in the case of

---

[1] Minutes of Proceedings Inst. C.E., vol. xlviii., p. 257.

Mr. Anderson.

chambers in a large building, there must be some means of shutting off the heat in each room independently of the heating apparatus. In such a case it would be found that the amount of water in the radiators being considerable, and the heat great, that it would take several hours to produce an appreciable difference in the temperature after the water was turned off; but when steam was turned off the temperature fell in a few minutes. The subject of the proper position of the heating apparatus, and the proper way of ventilating was so wide that it could hardly be dealt with in a discussion on the Paper. Most of the members would no doubt recollect that in Lancashire and other manufacturing parts of England, it was customary to put the heating pipes, generally steam, just above peoples' heads—about 7 or 8 feet from the ground, hanging from the ceilings. That was the right way of heating if it was desired to avoid draughts. If the apparatus was on or under the floor, there must, of necessity, be upward currents near the radiators; it followed therefore that there must be downward currents elsewhere, generally by the cold walls and windows, to which persons were very sensitive. Those downward currents were usually ascribed to leaky windows, especially in churches and other buildings not much used and only occasionally heated, but they were, as a rule, only the currents of local circulation sufficiently strong to produce those unpleasant draughts with which all were familiar. If the heating took place overhead, there was a thick layer of hot air above which did not tend to come down, and which warmed the ceiling and the walls, and the radiation thence warmed the lower part of the room; in that system, however, was involved a difficulty in providing ventilation. In fact, the subject bristled with difficulties in all directions. Exhaust steam from an engine had been used for heating purposes at the General Post Office. There was a difficulty in getting sufficient water for condensing the steam of the engines used for working the pneumatic telegraph despatch. The water from the hot-wells was therefore passed through a large coil, which was thus used for heating the staircase, and at the same time cooling the water. The plan had answered successfully, and it had solved a question that had been raised—whether there was any use in attempting to heat a building with water at a temperature of only 120°—because the staircase in which the coil stood was well warmed by water at that temperature. Unfortunately the rate of emission of heat was so slow from the hot-water pipes that it would require two or three coils of the size of the one already erected, which had 3,000 square feet of heating surface and

Mr. Anderson. weighed 18 tons, in order to dissipate all the heat which escaped from the engines.

Mr. Butter. Mr. H. J. BUTTER observed that the carriage department workshops at Woolwich Arsenal during the last twenty-five or thirty years had been warmed by means of boilers, which had become too old to maintain the proper amount of steam-pressure, and had been abandoned for that purpose. The boilers, when in an efficient state, worked at 60 or 70 lbs. pressure per square inch, but when they were too old for that they were kept to raise steam at 15 or 20 lbs., and it was then circulated through the warming tubes. The practice had been carried on with great success, and the workshops were made very comfortable. The pipes were of cast iron, 4 inches in diameter, and were connected at the ends with wrought-iron pipes $\frac{3}{4}$ inch or 1 inch in diameter, so as to ensure the circulation from the higher to the lower pipes, and cause bends, when required, to be easily accomplished. In some cases the condensed water returned by gravity and circulation, to the boiler; in others, where it was not convenient to use double pipes, the condensed water was conducted away into a drain. The metallic joint alluded to by Mr. Anderson was one that he had himself successfully tried for various purposes. Mr. Fowler's gas-retort lids, perfected by Mr. Holman, were largely in use, enabling the retort to be closed by a metallic joint, and these were of the same character as the pipe-ends mentioned by Mr. Anderson.

Mr. Robins. Mr. E. C. ROBINS had been requested to visit Belgium with a view of examining some high-pressure heating apparatus in that country, and he was accompanied by some professors who were anxious to determine certain points in connection therewith. He saw at one manufactory (J. L. Bacon, Antwerp) certain experiments carried out in which an extremely high pressure was put upon the pipes, but they stood everything that could be done to them. He particularly alluded to the joint used by Perkins, Bacon, and others, which had been tested up to 5,000 lbs. per square inch. For his own part, he had some hesitation in regard to the mixing of cast-iron with wrought-iron work. The increased number of joints required by the introduction at every turn of the coils of the particular joint, recommended by the Author of the Paper, seemed to be unnecessary if the wrought-iron could be bent easily, as it could be cold or hot, into any shape that might be required for the purpose of making coils. The subject of heating houses was no doubt extremely difficult in consequence of its being mixed up with the question of ventilation. One thing, however, was certain, that so long as pipes were exposed to the air

Mr. Robins.

in rooms there would be a great quantity of dust accumulating upon them. That dust was rarely removed, and it gradually became a coating; in certain states of the atmosphere moisture was found to cling to it. After six or nine months of warm weather, the apparatus was heated for winter, and then the heat was applied to a coating of material, which produced a faint unpleasant smell, and led to the complaints about heating by pipes, which were so common. He thought that no system of heating pipes could be tolerated in houses without a proper system of ventilation, giving a sufficient amount of fresh air, and effecting a complete change two or three times in an hour. With such a system the air might be let in and let out almost when and where they pleased. In visiting the large hospital lately finished at Antwerp, he observed that the principle was, to allow the heated fresh air to come up from below through the head of the capitals of the eight columns, in the centre of the circular blocks, to pass over to the outer wall, and to go out through gratings at the floor level between the beds to the extracting shaft. It might seem an odd thing to bring the expired air close to the beds, but the fact was the air was never allowed to get so bad as to become prejudicial to health. There was a constant flow of fresh air mingling with the warm, and equally constant extraction thereof, so that there was never any really bad air to be got rid of. The question was no doubt a difficult one, and whether this was a solution still remained to be proved. At the time he visited the hospital in question the beds were not occupied by patients. He had also visited a number of polytechnic schools in Germany, where the fresh air from below was passed in through stoves heated by steam, water, or fire, then flowing upwards was drawn down the opposite side by the extracting flues at the floor-level. In some of the schools there were chemical laboratories which required two kinds of ventilation to be going on at the same time, one for general purposes, and the other for the removal of smells from the "stink closets." In many instances the two worked against each other, and their effects were neutralised; to avoid this at Geneva the general ventilation was shut off.

Mr. Bridges.

Mr. LYMAN BRIDGES said that a previous speaker had severely criticised the system of heating and ventilation adopted in America. He wished to remark that some buildings in America heated by steam were poorly ventilated, and others well ventilated, and he presumed that there were similar differences in London. There was in New York one building that, in the summer, had 12 tons of ice in the basement, through which a column of air was sent

Mr. Bridges. to be cooled from 98° to 60°. That was a kind of ventilation that he had not heard of in London. The fact was that the owners of buildings would not permit architects and engineers to go to the necessary expense. The question of direct and indirect radiation had occupied a great deal of the time of engineers and architects in America. The system of having a succession of chambers of coils with low-pressure steam, or hot water, as it was there called, in different parts of the building, in the basement or cellar, all supplied from one source, receiving the air directly from the outside, and sending a large volume of pure warmed air vertically through flues to controlled registers in rooms above where it was wanted, had been adopted in preference to carrying a large number of cubic feet of air horizontally. That method had also assisted in the ventilation; in many instances there were ventilators at both the upper and lower portions of the rooms or halls. In recent buildings of the better class the system of indirect radiation had sometimes been adopted, but in older buildings, where steam had been introduced, direct radiation was almost compulsory, and where, as in some offices, but a very few hours' heat was required, direct radiation was most economical. Notwithstanding the remarks of Captain Galton, he thought that generally the ventilation had kept pace with the heating. There was in New York a company which had circulating pipes through the streets for heating, cooking, and supplying power for elevators. Sixty boilers were completed, and nearly the whole of one ward in the city was well supplied, each boiler having its own district. The return-pipe was about half the size of the supply-pipe, and the return-steam was about one-third of the heating capacity of the steam sent out. The system had been sufficiently tested to prove its practicability, but its financial success had not been demonstrated.

Sir Frederick Bramwell. Sir F. Bramwell asked Mr. Lyman Bridges how the charge for the steam was assessed, and how the deposition of moisture was prevented when the air was cooled by its passage over ice?

Mr. Bridges. Mr. Lyman Bridges replied that the inhabitants paid according to the quantity of registered steam they used, but he did not know the details of the method adopted for ascertaining the quantity. With regard to the passage of air over ice, he might mention the temperature was regulated with the utmost delicacy in summer and in winter, the arrangements being very complete for producing the same temperature.

Mr. Atkinson. Mr. W. Atkinson thought that the air, instead of being moistened, would be dried by passing over ice. In hot weather the air contained a great deal of moisture, and as it was cooled it deposited the moisture on the cooling surface. Mr. Atkinson.

Mr. T. H. BLAKESLEY said that the successful result of heating a passage by an apparatus at a low temperature would create no surprise to those who were familiar with the laws which had been quoted by Mr. Anderson. At a low temperature, the proportion of heat given out by radiation was far greater than that given out by convection, so that in a passage a good deal of draught was avoided by the walls being heated by radiation. Sufficient attention had not, perhaps, been bestowed upon the difference to which he had referred. At about 73° of difference, the air being at 60°, with an apparatus of cast iron the proportion was nearly equal, but below that the amount given out by radiation was in excess of that given out by convection. Mr. Blakesley.

Mr. W. S. LOCKHART said it would be interesting to know how in America moisture was added to the air after it had been heated so that it might not be unpleasantly dry. In the House of Commons there was an arrangement by which a spray of fresh water was thrown on a hot pipe, so that it was at once flashed into steam and carried away by the air passing into various chambers. He thought that was a much better system than the more frequent one of using water in open pans, which was boiled and boiled again, generally producing an unpleasant smell. Mr. Lockhart.

Mr. L. W. LEEDS remarked that he had been much surprised, on first visiting England three or four years ago, to find how little had been done towards what Americans termed "domesticating steam"; using it, in fact, for securing personal comfort, the warming of houses, cooking and ventilation. Steam appeared to have been tried in England for these purposes soon after it came into general use for mechanical purposes, but certain inconveniences were found in applying it to dwellings which caused its disuse. Much greater progress had been made in its general introduction in America; and the Author was one of those who had devoted their talents to the completion of those minute details essential to the popularizing of this method of heating. Mr. Leeds was inclined to attribute the comparatively high death rate in large European cities, such as London and Paris, in winter, to insufficient artificial warmth. It was noticed during the siege of Paris that the want of food did not have that decisive effect on the mortality which was caused by the want of artificial heat. In America, on the contrary, where houses were generally kept 10° or 15° warmer than here, the death rate was lower in winter than in summer. He accepted Captain Galton's criticism that steam-heated houses Mr. Leeds.

Mr. Leeds. in America frequently gave rise to unpleasant sensations; but this was owing to imperfect ventilation. He thought the evil might be remedied by putting a steam radiator into the fire-place instead of an open fire, and leaving the chimney for ventilation as before for the escape of vitiated air. Heating with steam by direct radiation from exposed steam pipes gave the opportunity of sealing a room and raising the temperature to any desired point, and also of avoiding in a great measure disagreeable draughts. Want of attention to the proper supply of air was what had so disgraced this system of heating. The open fire could not be thus abused, and was consequently the safest in the hands of ignorant people. He held that heating was one thing and ventilating an entirely distinct operation, but that both must be carried out in the same room. The open fire was admitted to be the pleasantest and most healthful source of warmth, but the inconvenience of keeping up the fire and the unequal distribution of the warmth left much to be desired for the satisfactory warming of many buildings. The point to be arrived at was a well-distributed warmed surface which could be kept thoroughly clean and free from dust, and be at the same time easy of control. Steam offered these advantages in a greater degree than any other agent yet known. The system of heating by hot water had some apparent merits, one advantage being the lower temperature of the pipes, and the greater range of temperature, which might be regulated to meet the various changes in the external atmosphere. The steam-heated surface did not possess this feature directly to the same extent, but with a little ingenuity this disadvantage could be well-nigh overcome. There were various ways of regulating the heat; first in each radiator, and secondly by the use of portions only of the heating surface in each building. For instance, in mild weather it was frequently sufficient to turn steam on to the radiators in the passages only, or to a single radiator in a room. Other advantages were the rapidity with which steam circulated to the remotest corner of any building or any ordinary row of buildings, and the ease with which the heat might be turned on or off, as readily in fact as gas or water. In the latter respect it had the advantage of hot water, which took a long time to cool, in case of overheating, whereas the steam radiator cooled quickly when closed. He had experienced no difficulty in maintaining a continuous fire, and so keeping up a uniform pressure of steam. He had set the apparatus to work at 5 lbs. pressure per square inch, and left a good supply of coal, and the same uniform pressure had been maintained for thirty or forty hours. At Krupp's works

Mr. Leeds. at Essen it had been found economical to concentrate the boilers in one place and to distribute the steam in all directions for about 2 miles. In this case the steam was conveyed along the streets in large pipes overhead, with very little protection from the external atmosphere. The economy of this method of generating heat in large quantities would be just as great for domestic purposes, and it would be much cheaper for individuals than where each family was obliged to maintain a separate fire. No block of industrial dwellings ought to be erected without its single central fire for supplying steam for all the warming, cooking, washing, ventilation, and any power that might be needed; and for hospitals and public buildings the intelligent and judicious use of steam would be found the most suitable for doing all this work.

Sir Frederick Bramwell. Sir **Frederick Bramwell**, Vice-President, said he had had the privilege of knowing the Author of the Paper, and it was a source of the deepest regret to him and to all the other friends of the Author that the Paper should have to be read after his death, but so it was, and thus the members were deprived of his presence and of the information he could have given, and also of the defence he could have made of the views put forward. In the early part of his Paper the Author stated that Mr. Nason, who was a pupil of Perkins (the grandfather of the present member), in England, in 1840, had intended when he went back to America to take up the hot-water system that Mr. Perkins had then introduced; but it would appear that what Mr. Nason did take up and propagate was heating by steam. Why he abandoned Perkins's system of heating by hot water the Author of the Paper did not mention. Sir Frederick Bramwell did, however, know from his own personal experience that heating by hot water, as practised by Perkins, was in the highest degree satisfactory. He had had it at work for five years in his present house, and for seven or eight years in a house he had previously occupied, and had never had the slightest trouble with it. The present was the third year during which the fire had been lit without a single pint of water being put into the apparatus, which was manageable in every way. The best manner, he thought, of controlling the different sections was not to attempt to shut off the circulation and to have a by-pass, but was the simple one of putting the coils into cases and fitting each case with a sliding top, so that when the heat was not wanted the sliding top could be shut, and then, although the water circulated through the coil inside, no heat was given forth. The Author went on to give certain details (and it was with details that his Paper principally dealt), those most useful of all useful things to enable engineers to carry

Sir Frederick Bramwell. out their works effectually. Among the details referred to was the mode of making the joints of the pipes used in steam-heating in the United States; but he concurred in the regret expressed by Mr. Anderson, that the system introduced by a gentleman who had been a pupil of Perkins's should have departed from the absolute simplicity and effectiveness of Perkins's plan, the contact of metallic surfaces, and that, instead of following it, he should have employed conical threads and the red lead and white lead cement. He should have been glad if the Mr. Perkins of to-day, the grandson of Mr. Nason's master, had been present on this occasion. In his absence it might be stated he had expressed a wish, in which Sir F. Bramwell heartily concurred, which was that red and white lead were a guinea a pound, because if they were there would be an end of the smearing mess, and the work would be good. The reason why the apparatus which he had of Perkins's make had not wanted a drop of water for three years was that the joints were absolutely metal and metal. There was no difficulty about them; an ordinary workman fitted them up. They were made once and made forever, and it did seem to him a matter of regret that in America this plan should not have been adopted instead of the system of conical threads, and the old-fashioned red and white lead cement of which engineers' men were so fond. The Author of the Paper had given credit to Mr. Nason for the introduction of what he called the close circulation of steam-heating by which steam might be used below atmospheric pressure. The same pipe led out of the boiler, went about the building to be heated, heated it, and returned the condensed water into the boiler. That system was pretty nearly as old as himself; at all events when he was apprenticed, his master used it for the purpose of heating and drying gunpowder at Waltham Abbey. The apparatus consisted of a steam boiler delivering its steam into a series of vertical pipes having a provision to pass a current of air around them, and placed below a sieve, upon which the powder was spread, the condensed water flowed back into the boiler. Thus the close circuit was to his knowledge forty-five years old. Another point touched upon by Mr. Anderson, to whose remarks he had before alluded, was the use of cast couplings for the wrought-iron pipes. Considering the ease with which wrought-iron couplings were made, he agreed with Mr. Anderson it was a pity to introduce cast iron into that which was otherwise a wrought-iron system. Mr. Briggs had given extremely good diagrams of the method of finding the proportions of certain parts. Such diagrams were very useful when, as here, they had been well thought out, Sir Frederick Bramwell.

and they would prove, together with the particulars in the appendix of the power of the apparatus to give forth heat, extremely serviceable, assuming always that those particulars were entirely trustworthy. Mr. Anderson however had referred to some experiments he had communicated to the Institution a few years ago, showing that it was wrong to assume the power of heating per degree of difference of temperature was the same for all degrees of temperature. The Author, as had been already noticed, had overlooked altogether the excellent work of Tredgold on warming and ventilation, apparently not even knowing of its existence. That work was based upon experiments carried on with the accuracy with which Tredgold carried on all his experiments, and he believed there was very little left for anybody to learn on this subject if he had made himself thoroughly master of Tredgold's theory of warming and ventilation. With regard to the Seguin boiler used in the United States—a cylindrical boiler with small tubes, and fire underneath returning through the tubes, the waste heat going back over the top of the boiler, there was very little doubt that boilers of that kind owed their use to the employment of anthracite, which, as Mr. Briggs had stated, would not submit to be touched. It must have a large fireplace, to lie in and burn as it liked, and that to some extent prohibited the employment in America of the internal-fire boilers which were used so much in this country. He was glad, however, to see at Mr. Sellers's works the other day that he was making Galloway boilers with internal flues and circulating tubes. The Author said, in the most "naive" way, one defect of the Seguin boiler was that the bottom plate occasionally came down. That was precisely what he should expect with a boiler fired underneath, with the intense fires due to anthracite. With reference to the statement in the Paper about heating with exhaust steam, every sugar-house, when he was a lad, was heated with exhaust steam from the non-condensing engine, and when that was not sufficient it was supplemented with live steam. That also was no novelty of any kind. So, too, with regard to the question of the supply being by a single main, every one of the sugar-houses to which he had referred was heated by a single main laid at such an inclination that the condensed steam could flow along the bottom of the pipe, and find its way back into the boiler or into a steam trap. He should have been glad if the Author could have given the details of Professor Mapes's steam-trap and of the Albany steam-trap. There was a little difficulty about steam-traps, but he believed it had been overcome by the steam-trap of Robertson. He knew that in his young days when

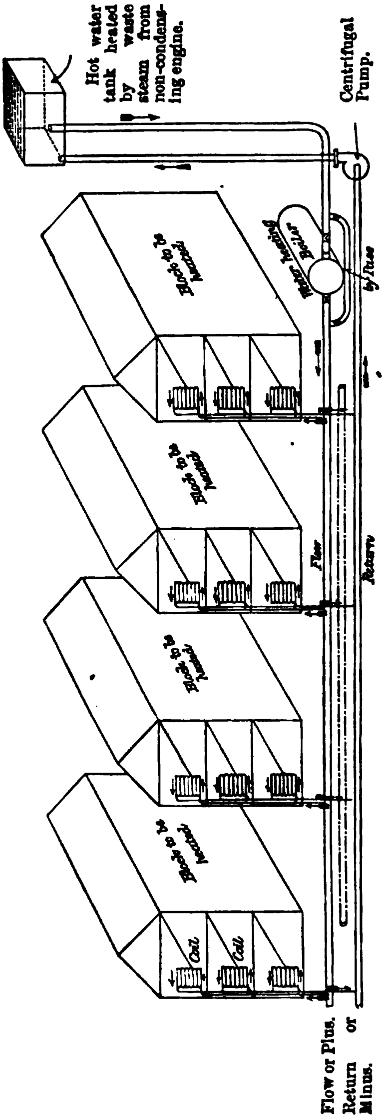
Sir Frederick Bramwell. steam-traps (then called siphon-boxes, as they replaced the old siphons used with low-pressure steam) were attempted they required to be made very tight, and if the coppersmith's work leaked the trap failed. Robertson's plan was to work by the leakage, as it were, of the trap; it was made of cast iron, and, so far as he knew, its performance was perfect. Mr. Briggs had gone but slightly into another point, which he thought after all was the important one, namely, the connection of ventilation with heating. There was no doubt if there were not that connection, then, so far from the introduction of steam into dwellings for the purpose of warmth being a benefit, for which the introducer ought to be praised, it was the most pernicious present that could be made to any one. By closing the door and the window and turning on steam into a coil of steam-pipes, the persons in the room having only vitiated air to breathe, would, notwithstanding a high temperature, feel an ever-increasing need of more heat, and thus the temperature was raised higher and higher, until the thermometer, as was stated in the Paper, was at 85°. That was the temperature at which sugar-loaves used to be dried; and when to the want of ventilation was added the habitual burning of gas, it was impossible to conceive anything more hateful and more prejudicial than an apartment so dealt with. Mr. Briggs said that there were three modes of dealing with heating by steam. The first when there was no ventilation at all; the second where there was some ventilation, and the third where there was good ventilation. He was sorry to say that the simplicity of the one in which there was no ventilation, the economy at the outset, and the economy in the working, were great temptations to the class of persons who had been alluded to by the last speakers, those not very well off. They had no knowledge of the necessity for ventilation, and the result was the ventilation was not good, and he believed in such cases the system did infinite harm. Even a smoky chimney was more desirable, because more healthful than was a smokeless close room, coupled with a gradual rising of the temperature, between steam and gas, to 85°. Much as he liked that which he saw in America, there was one thing about it that he did not and could not like, namely, that it was a place where it was certain there would never be in a room a temperature below 70°; for if Providence did not send it man turned it on. For himself, he felt happiest in a temperature of about 62°. He could not live comfortably when it was much higher, and if the high temperature was accompanied by want of ventilation, he was completely "done for." He held it to

Sir Frederick Bramwell.

be most undesirable that there should be a possibility of making a great heat without a certainty of ventilation. Mr. Briggs had said that the habits of Americans required, for their health and comfort, that the temperature should be from 70° to 80° or 85°, and that this requirement, and the difference in the conditions of climate would render it useless for him to give any statement as to the proportions of surface and of boiler power that were needed for the purposes of heating buildings in England. He had, however, given such information as was independent of these considerations, namely, what, according to his view, the number of units given forth would be by any particular apparatus, and in that respect his Tables would be most useful. By means of those Tables, taking into consideration the temperature prevailing in England, and being guided also by the observations made by Tredgold, he thought it would be easy to lay out any apparatus required for use in this country. He should like to mention two instances of heating. One was at Sir W. Armstrong's house, Cragside, where he had had the pleasure of being during an autumn, and where, by a combination of open fires and heating apparatus, which seemed to be everything that was desirable, draughts were unknown, excessive heat and cold were unknown, while the cheerful, open, "pokable" fire in which he still believed, and which he hoped no one would take from them, was also to be found. He thought he was right in his recollection that Sir William Armstrong's arrangements were such as to allow the apparatus to go on for twenty-four hours without any attention. It stoked itself, and kept itself at any temperature that might be desired. Another mode of heating which he wished to notice was, he believed, only to be seen at one place, and with reference to this he desired to direct attention to two diagrams (Figs. 9 and 10). The place heated was an aggregation of several blocks of buildings having an extreme measurement of 725 feet wide and 900 feet long. It was heated by open fires to ensure ventilation and comfort in appearance, but these were supplemented by coils. The whole of the coil heating was derived from a central source of heat consisting of a number of boilers placed close by the engine-house. The system was arranged thus: In a tank 78 feet above the ground was a heater, into which the waste steam entered from a non-condensing engine that raised the water from the well and did the other work of the place; it heated the water not by being blown into it, but by being transmitted through surfaces. In winter time the partially heated water descended by a pipe, entered the boilers provided for the purpose, was further heated

Sir Frederick Bramwell. there, and then issued out of them, along a pipe to the end near the word "plus" in the diagram, which was a stopped end.

FIG. 9.

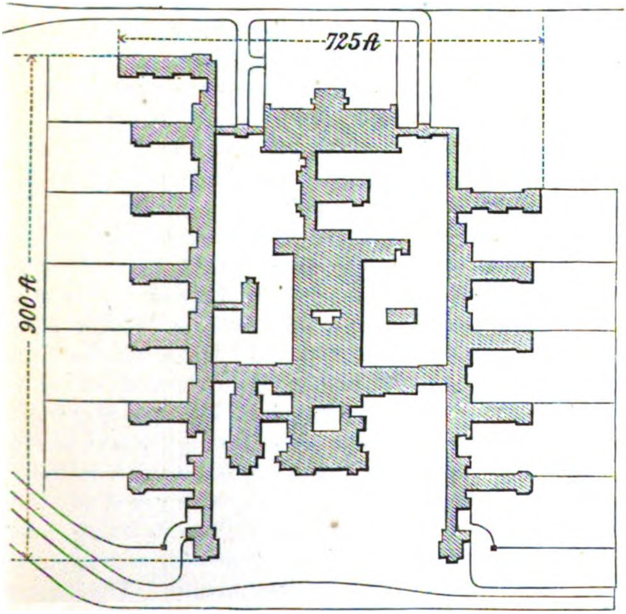


Parallel with that there was a return pipe which had a stopped beginning near the word "minus;" that return pipe came to the

Sir Frederick Bramwell.

suction of a centrifugal pump, and the centrifugal pump delivered into the tank. It would be seen that if everything were closed and the pump was not at work there would be the pressure in both pipes due to the 78 feet elevation of the tank; but if the pump were started there would be a difference between the pressure in the flow and that in the return pipe, due to the velocity at which the centrifugal pump was working, and this pressure was found to be about 6 lbs. to the square inch. There was no waste in the use of the pump, other than the conversion of a certain very small amount of heat into power, because the exhaust steam from its engine went along with that of all the other engines to do the heating. Having thus established a minus pressure in the one pipe, it was perfectly easy in coming opposite any one of the blocks of buildings to take a flow pipe from the plus pipe into the building, to circulate this pipe through as many coils as were wanted, and then the function of the pipe changed from a flow to a return, and was taken into the main pipe wherein was the minus pressure. They then had as the power to produce a flow through the buildings, not merely the feeble pressure of a head equal probably to 6 or 8 inches of

FIG. 10.



Sir Frederick Bramwell. water due to the difference of temperature between the rising an the falling column of water in the building, as in the case of a ordinary hot-water circulation, but the pressure due to the actio of the centrifugal pump. A centrifugal pump was one specially suited to this purpose, for by its employment even if all the cocks were shut off no harm would arise; the utmost that would happen would be that the one pipe would have the full pressure minus the pressure due to the action of the pump. The pressure in the one pipe was about 33 lbs., and in the other 29 lbs.; but by reason of that pressure there was a certainty that the water would circulate through any number of coils in any of the buildings; and it circulated with such rapidity in the buildings in question, which were 900 feet long, that when the water got back into the engine-house it had only lost 30° of temperature. Leaving at 230° it came back at 200°, producing, therefore, a very great effect on the various heating surfaces. Between the two pipes there was a third pipe, shown in dotted lines in Fig. 9. There was a group of valves at each of the places where a set of flow and return-pipes came off, and by means of those valves any one of the three pipes might be a flow-pipe, and any one might be a return pipe; thus one of the three might be broken and the circulation would still be kept up unimpaired. It should be stated that a by-pass was provided at the boilers, so that in summer, when the hot water was only needed for baths, and for cleansing purposes, and when the waste steam from the engines provided sufficient heat, the flow could take place directly from the elevated tank without passing through the water-heating boilers, which at that season of the year were not fired. The system had been in operation for four or five years, and there had been no trouble with it of any kind. The building was being largely extended, and the same plan of heating was applied to the extension with entirely satisfactory results. That method of heating, from a sanitary point of view, had many advantages. There was a large body of water which kept up the heat during the night, and there was perfect control of the circulation through the building. In conclusion he desired to say he was one of those who were most desirous that hot-water heating, or steam-heating should be largely introduced, coupled with a certainty of ventilation, and, best of all, coupled with open fires, ensuring both ventilation and cheerfulness.[1]

---

[1] It would be seen that the two parallel pipes unconnected directly the one with the other, the one pipe having a + and the other a − condition (at least −

Mr. ARTHUR RIGG said that as the system of heating by pipes on the floor was most commonly used in England, he thought it might be interesting to the members to know the results of the employment of a system identical with that proposed by the Author—heating by pipes overhead. Mr. Rigg.

In the new Printing Works of Messrs. Unwin Brothers, which were heated by exhaust steam, he had persuaded them to have the pipes overhead, instead of on the floor according to the usual plan in such establishments. It was a building of several storeys, the top floor being occupied by compositors, who of all men were the most particular in regard to heat. They liked to live in a sort of oven, and when the system of placing the steam-pipes overhead was first suggested they said it would not succeed, and he was afraid that they would fight against it however good it might be. In the result, however, they were perfectly satisfied, and that, he thought, showed that, as far as heating was concerned, the arrangement was good. He did not think that the ventilation was particularly satisfactory, but that was not his fault.

The area of the upper floor was 3,530 square feet, and the capacity 45,398 cubic feet, including roof. The length of the 4-inch pipes was 228 feet, and the mean height above the floor 9 feet 6½ inches. The slope was 9 inches in the entire length in the direction of the current to carry off water. There was thus a length of 1 foot of pipes to 200 cubic feet of room, and that seemed to be ample. The area of the walls, windows, and the roof, all exposed to the outer air, was 6,908 feet, and dividing that by 228 gave a proportion of 30 to 1.

These particulars seemed to show that, for a building exposed on all sides to the weather in such a climate as London, for an elevated range of pipes heated by exhaust steam at about atmospheric pressure, sufficient warmth would be obtained by providing 1 linear foot of 4-inch pipe, equalling 1 square foot of heating surface to 200 cubic feet capacity, or to 30 square feet of wall and window surface and roof exposed to the outer air.

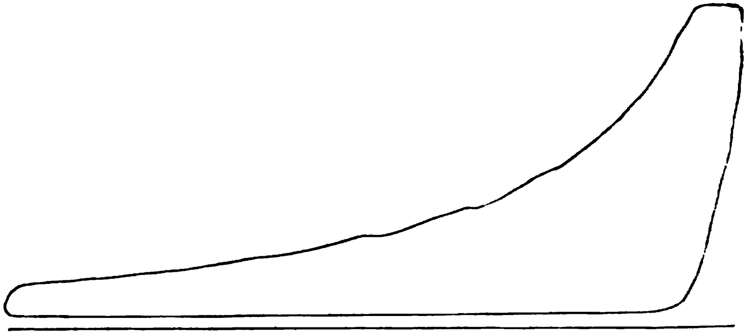
He was curious to see the effect in back-pressure upon the engine by sending the entire exhaust steam through one set of pipes in one room, and he had had some indicator diagrams taken for that purpose (all to a scale of $\frac{1}{30}$ inch = 1 lb.), which showed a very

---

as compared with the +), and the uniting these pipes at each block of buildings by a pipe proceeding from the + through the heating coils in the building and joining up to the − was similar to the arrangements in electric lighting of incandescent lamps coupled up in parallel arc.

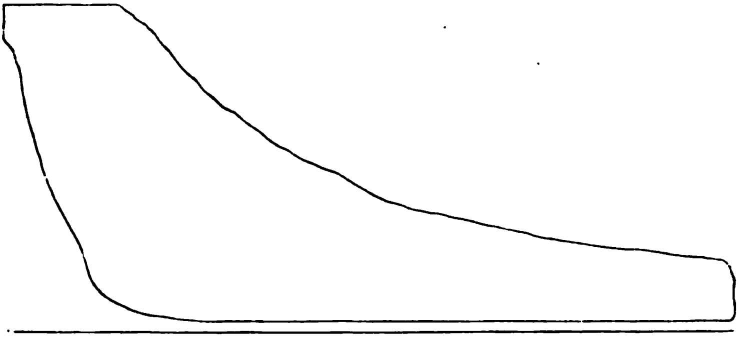
Mr. Rigg. small amount of difference. Figs. 11 and 12 were taken when the exhaust was open and the heating apparatus shut. Figs. 13 and 14 were taken when the heating apparatus was open and the exhaust shut. There was about ½ lb. per square foot extra back-pressure on the exhaust, while the steam passed through the heating apparatus of one room only, but as a general rule there was not even so much, because the steam when driven into one room was allowed to escape partly through the exhaust and partly through the heating apparatus.[1] That application of the system Mr. Briggs had brought forward, he thought, was rather successful.

FIG. 11.



CYLINDER END NEAR CRANK.

FIG. 12.



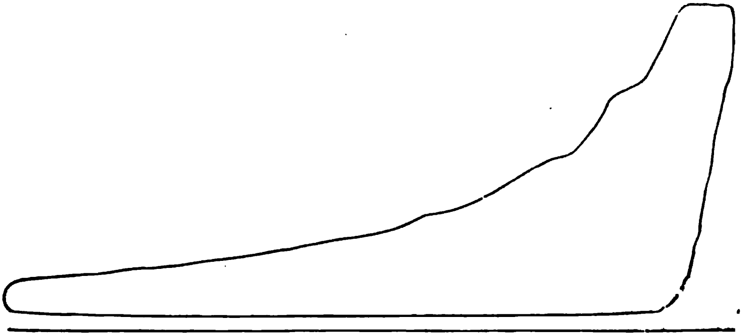
CYLINDER END FROM CRANK.

---

[1] This engine had a cylinder 12 inches in diameter, a length of stroke of 20 inches, revolutions 130 per minute, and 29 HP. indicated by the diagrams.

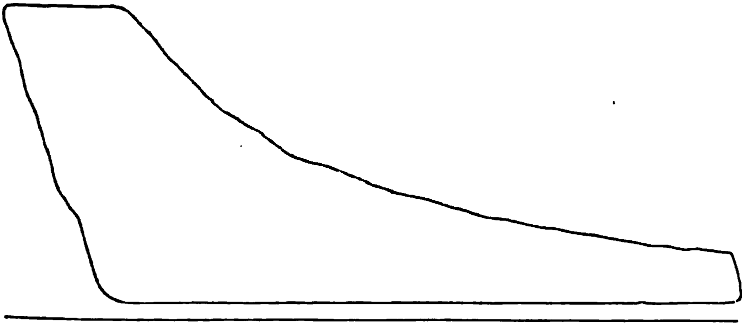
With regard to ventilation that was a matter that had not been sufficiently attended to in this country. It was generally said that hot air was lighter than cold, and that was perfectly true; but the fact was that it was not hot air alone but a mixture of air and carbonic acid gas that had to be dealt with. The proportion in weight between carbonic acid gas and air was as 3 to 2, and taking air at a temperature of about 60°; then in order to make carbonic acid gas the same specific gravity it must be heated to over 300° Fahrenheit. Of course the proportionate weight of the mixture, whatever it might be, would be a mean between these two, so that when there was heated air in the top part of a room with a large quantity of carbonic acid gas, as it cooled the heavier gas naturally tended to fall, and there was not such a great difference as was generally supposed between the weight of a cubic foot at the top part of **Mr. Rigg.**

FIG. 13.



CYLINDER END NEAR CRANK.

FIG. 14.



CYLINDER END FROM CRANK.

Mr. Rigg. the room and at the bottom. In a large room—in a church, for instance—it would be found that after a certain time the carbonic acid gas seemed to descend like a pall and produce a feeling of stupor. Of all buildings none were better adapted for ventilation than churches where there was a large area above and ample accommodation for the disposal of draughts instead of having them below, and yet there were no places worse ventilated in the country.

Sir William Armstrong. Sir WILLIAM ARMSTRONG, President, said his small experience upon the subject of the Paper was pretty nearly confined to his own house, which was heated by hot water of moderate temperature in large pipes, and the house of a friend, which was heated by high temperature and small pipes; and he must say that his friend's house always smelt like an engine-house, while his own was entirely free from that objection. He did not know what it was that caused the unpleasant stuffiness which always seemed to accompany highly-heated pipes. Probably it was owing to those minute particles of organic matter floating about in the air, which were conspicuous when a beam of light streamed through a room, coming in contact with a highly-heated surface, and thereby volatilised, and so producing the unpleasant effect to which he had referred. No doubt that would be overcome by ample ventilation, but ventilation implied carrying off and wasting a great deal of heat. His own impression was that the best method of heating was to have larger pipes, a larger surface, and a low temperature, and that ventilation should be obtained by bringing air into contact with the pipes before it entered the room. Sir F. Bramwell had referred to his apparatus at Cragside. He was very conveniently situated there for self-acting arrangements. There was a high hill immediately behind the house and a fine spring of water which was brought down in a pipe that gave a considerable pressure at the house. By applying it to a little hydraulic apparatus he worked a Jucke's grate, and the rate at which it worked was regulated simply by the tap upon the pipe. On a cold day the tap was opened freely, and on a warm day it was contracted and so regulated the feed of fuel upon the fire. The form of the boiler was that to which Sir F. Bramwell had referred, a form used extensively at the Elswick works, consisting simply of a cylinder to which the fire was applied beneath with tubes to bring back the flame and heated air to the opposite end. It worked exceedingly well, and he had not seen any mode of heating that worked more satisfactorily. At the same time his experience in the matter was not sufficient to give much weight to his opinion.

Correspondence.

Mr. Bache.

Mr. Alfred Bache drew attention to a Paper[1] on "Steam Heating," read in America in June, 1882, by Mr. Charles A. Smith, member of the Engineers' Club of St. Louis, from which some of the practical information and of the conclusions arrived at might usefully be quoted by way of supplement to the Paper under discussion.

As to the prevalence of warming by steam in American towns, Mr. Smith gave particulars of five towns in which there were altogether 13 miles of underground steam-pipes, ranging from 10 inches down to 1 inch in diameter. In Dubuque, Iowa, with 3½ miles of pipe, there were one hundred consumers, and 3,500,000 cubic feet of space were heated by an evaporation of 25,000 lbs. of water per hour from 40° to 280° Fahrenheit; but about one-half of the steam was wasted. The loss of heat from the pipes, as usually laid and protected, was 50 British thermal units per square foot per hour, with the steam at 258° and the ground at about 58°, the weight of the water condensed per hour being 0·05 lb. per square foot.

The boilers were worked most like locomotive boilers. It was necessary that they should be able to stand firing hard; because this practice was more economical in very cold weather, at the expense of fuel, than to incur the cost of providing larger boilers yielding higher duty, which would be working below their power in less severe times.

For rapidity of heating, the system of direct radiation from an ample extent of surface appeared best; but for steady heating, combined with purity of air, the system of indirect warming by currents of air was preferable.

For warming rooms in mills, the most recent practice, as recorded by the Author, of placing the direct radiating pipes in rows overhead, suspended from the ceiling, was not endorsed by Mr. Smith, who recommended for economy the older and more general plan of carrying the pipes along the walls and near the floor.

The question of warming with live steam or with exhaust steam was discussed at considerable length by Mr. Smith; and the conclusion arrived at by him was that, with non-condensing engines, or with the best class of condensing engines using superheated steam and worked non-condensing during the winter,

---

[1] Journal of the Association of Engineering Societies, Sept. 1882, pp. 369–380.

Mr. Bache. it was desirable to use their exhaust steam rather than to provide a separate boiler for the warming apparatus; but that with ordinary condensing engines a separate boiler for warming was preferable.

Mr. Dowson. Mr. J. E. Dowson remarked that although the Author of the Paper had given useful data for the practical fitting up of a system of steam heating, he had not entered into a comparison of such a system with heating by coal or gas fires. In this country coal was cheap and its use prodigal, but economy and the abatement of the smoke nuisance were nevertheless present in the minds of manufacturers and householders. If, therefore, steam heating could be shown to be more economical and efficient than other systems, the sooner it was known the better.

Before, however, coming to any decision on the subject various points must be considered; and briefly some of the pros and cons of the three systems might be thus summed up:—

ADVANTAGES.	DISADVANTAGES.
Steam.	*Steam.*
No smoke or dirt. No products of combustion except at the boiler house. No risk of fire. Can be turned on or off as required.	Loss from condensation in a given length of mains is constant, whether there are few or many consumers. Ventilation of rooms generally unsatisfactory. Unsuitable for many kinds of cooking. Air often too dry. Condensation deposited on walls, &c., if air hotter than walls. Cost of distribution great. Risk of bursting boiler and pipes.
Coal fires.	*Coal fires.*
Cheerful appearance. Suitable for all kinds of cooking. Walls of rooms, &c., heated by radiant heat without air being too dry. Ventilation good.	Much smoke and dirt. Great loss of heat and unconsumed fuel up chimney. Risk of fire.
Gas.	*Gas.*
No smoke or dirt. Can be turned on or off as required. Suitable for all kinds of cooking. Walls of rooms, &c., heated as with coal fires. No appreciable loss by condensation.	Ventilation liable to be neglected. Cost of distribution great, but much less than for steam. Loss of heat up chimney, but much less than with coal fires.

From the above general considerations it would appear that the disadvantages of steam heating outweighed those of coal fires or

Mr. Dowson.

gas. The abatement of the smoke nuisance was doubtless a strong point in its favour, but gas was equally efficacious in this respect. There remained, however, for consideration the important questions of efficiency and economy.

It was not possible to make an exact comparison of the economical results of the three systems without going closely into numerous details, such as the construction of each heating apparatus and the conditions under which each was worked. In general terms, however, he ventured to make the following remarks.

By calculation there were about 15,000 heat-units in 1 lb. of average anthracite. The Author stated: "It is accepted that 1 lb. anthracite of average quality will give out 9,000 units of heat to the steam generated." This was 60 per cent. of the total heat-units in the fuel consumed, or a loss of 40 per cent. The loss due to condensation was said to be trifling when the mains were very carefully jacketed, but it was not said what proportion of the heat taken up by the steam was returned usefully in actual practice. In the appendix several calculations were given, which had been based on the supposition that all the latent heat of the steam was utilised; but so high an efficiency was not probable in practice, and it was much to be regretted that so important a qualification had not been dealt with.

In the absence of the necessary data it was impossible to make an exact estimate, but seeing that only 60 per cent. of the total heat-units in the fuel consumed were taken up by the steam, that there must be some loss from condensation in the mains and service pipes, that there was probably some loss at the boiler house owing to the varying demand for steam in houses, &c., and that it was not reasonable to assume that all the latent heat of the steam was utilised, he thought it favourable to suppose that 30 to 35 per cent. of the total heat-units were converted into useful work.

The proportion of heat-units utilised in open grates varied from 15 to 35 per cent., or an average of 25 per cent. of the total units in the fuel consumed. It would therefore appear that steam heating was about as wasteful of fuel as open grates.

Anthracite was much less burned in this country than in the United States, and many persons here would find it difficult to get reliable practical data regarding its employment. It happened, however, that for a long time he had used anthracite in gas generators, and he thought the following particulars would be interesting, as they admitted of a direct and simple comparison with some of the results given by the Author. Gas was made in

Mr. Dowson. his apparatus by passing steam and air through an incandescent mass of anthracite, and in this way the whole of the fuel, except ash, &c., was converted into gas. The gas thus produced contained about 85 per cent. of the total heat-units in the fuel consumed, showing a loss of only 15 per cent.

The proportion of heat-units utilised in gas-heated stoves of the best kind varied from about 75 to 84 per cent., or an average of 79·5 per cent. of the total heat-units in the gas consumed.[1]

It would thus appear that by converting anthracite into gas, instead of using it to make steam, an efficiency of 85 per cent. was obtained compared with 60 per cent., whilst with the gas there was no appreciable loss by condensation, and stoves could be used which had a proved efficiency of 75 to 84 per cent. As regarded the cost of production, the working expenses were about the same for gas of this kind as for steam, the only important item except coal being the fireman's wages, which were the same in each case. As to distribution the gas could be carried in ordinary *mains* which cost very much less than jacketed steam-pipes.

Messrs. Geneste and Herscher. Messrs. GENESTE and HERSCHER would premise their observations, based on Mr. Briggs's Paper, by drawing a brief comparison between the system he described and those in vogue in France. Among the details of execution to which the Author referred was the piping, which in America was nearly always of wrought-iron tubes, both for the steam- and the return-pipe. Great importance was, with reason, attached to the mode of coupling. Messrs. Geneste and Herscher thought that the conical-screw joint would be found very efficient. They employed another mode, which gave very good results. The two ends of the tubes to be coupled were threaded with right- and left-hand screws, as was also the socket by which they were joined; further, each tube-end was provided with a small jam-nut, Fig. 15, next page, of which the face that abutted against the edge of the socket when screwed home was concave; thus providing a sort of chamber to receive the excess of white-lead. This made a perfect joint. The sockets, elbows, branches, &c., used in France were always of wrought iron, and never of cast, as in America. For the condensation-pipes cast iron was very often used, with rubber joints.

While remarking that the Seguin boilers were generally employed in the United States, the Author referred to the multitubular boilers, generally called inexplosible. In France the

---

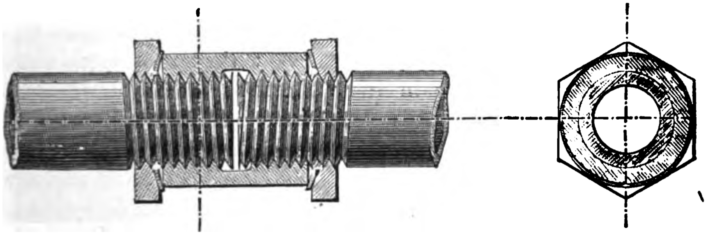
[1] Reports relative to exhibition of apparatus for the utilisation of gas, electricity, &c., held under the auspices of the Philosophical Society of Glasgow, September–October, 1880. 8vo. Glasgow, 1882.

employment of this type tended to become general, probably on account of its greater safety, but also because the much smaller quantity of water allowed of getting up steam much more quickly, while at the same time cessation of work involved a small amount of loss in heat-units from the cooling of a large mass of water.

Messrs. Geneste and Herscher.

Respecting the circulation of the steam, the Author indicated two modes; one with live steam, the other with exhaust steam. He described the two systems of open and closed circulation; as also the various devices used in the United States to effect the radiation of the heat. In France closed circulation was used to a very limited extent. Messrs. Geneste and Herscher gave the preference to open circulation. Their first care was to reduce the steam-pressure to such a degree as to be scarcely sensible when circulating in the places to be warmed. For this purpose pressure-regulators were used, which necessitated, especially in extended applications of the system, that the service should be divided into a certain number of secondary distributions. Live steam, at full pressure, was conveyed by a main to the sites of the various secondary distributaries. (If basements were available, or even mere underground tunnels, they could be used for the main line.) Before distributing the steam it was conveyed to the highest position in the building, and thence was circulated in such a way as to be constantly tending towards the bottom, but not reaching it until the pressure had fallen to a quarter of an atmosphere by pressure regulators. If basements were not available, the steam was at once conveyed from the generator at full pressure to the highest point of the building, the main being disposed under the roof, or in passages provided for the purpose, and thence to the secondary system as before. The entrance to each radiator was provided with a cock, so as to allow of shutting off the supply at will, and also with a steam-trap for the condensation-water. This trap was intended to avoid loss of steam, and at the same time to maintain the independence of the different heating surfaces. They attached great importance to

Fig. 15.



Messrs. Geneste and Herscher.

this last device, and were convinced that without the interposition of a good automatic trap to each radiator no efficient circulation of steam was possible. It was further necessary to apply a trap at the bottom of the ascending and descending pipes. On exit from the trap the condensation-water was received into a receptacle common to all, whence it was conveyed to a reservoir near the generators to be used again. They entirely agreed with the Author in his precautions to assure the circulation of the steam in the same direction as the condensation-water. They also agreed with his provision of frequent flange-joints to facilitate repairs.

The mode of lagging the pipes, attributed by the Author to Rumford, should give good results, and the sheet-iron casing, to avoid a too close contact of the felt with the outside of the hot pipes, was necessary. Without this precaution, felt was not to be recommended as an insulating material, not merely on account of its liability to destruction, but also because it absorbed, during the cooling of the pipes, a notable quantity of vapour, which, if the felt were in contact with the pipe, would be converted into steam when the latter got to work again. With respect to the globe-valve they had little to say, except that that type did not appear to them to unite in itself all the qualities desirable, but such a consummation was not achieved in any apparatus they were acquainted with. The valve shown in Fig. 4 appeared to be open to the objection that when it was closed cold, and steam was afterwards let in, the expansion of the screwed spindle would tend to press the valve against its seat to such an extent that it might be sometimes impossible to turn the screw until the globe got cooled again. This drawback might doubtless be remedied, but only at the expense of simplicity.

The Author's statements as to radiators had particularly interested Messrs. Geneste and Herscher, and had also surprised them. Even allowing (as did the Author) for the differing exigencies of comfort in America and Western Europe, they thought it a pity that ventilation should be for the most part entirely sacrificed; and in principle they maintained that it was essentially desirable (whenever possible, which was nearly always, at least in rooms, offices and schools) to combine a suitable degree of ventilation with the system of heating from radiating-surfaces. This combination was not specified in any of the three systems described by the Author. It was, in their own opinion, one of the advantages of steam-heating, that it lent itself more readily than any other system to efficient ventilation combined with local radiators fixed in the places to be warmed. This double condition did not

Messrs. Geneste and Herscher.

appear incompatible with the high temperature demanded by American ideas of comfort. Further on they would explain briefly the manner in which they effected the combination.

As regarded the convenient location of radiators in apartments, they were completely at variance with the Author, who would place the radiators against an interior wall. Messrs. Geneste and Herscher always disposed them against the coldest surface, and especially against any glazed partition. They believed there was no doubt as to this being the better plan, and that experience had shown it to be so.

They did not find anything to comment upon in the Author's construction of radiator, although, consisting of numerous assemblages of tubes, it differed notably from their own type. They would only mention that in the construction of radiators in general it seemed very desirable to avoid, as far as possible, inter-radiation (*rayonnement des surfaces les unes sur les autres*), so as to make the most of the radiating surfaces.

In the Author's third system, air was employed to convey the heat to the localities, the quantity of air furnished being so determined as to afford systematic ventilation. As was claimed for it, that system allowed of the co-ordination of warming with ventilation. Neither, when a blower was used, could an auxiliary heater be provided to warm the air to a small degree before it came to the heating-chambers. Nevertheless, the Author's system of combined ventilation and warming did not appear to be the best. In Messrs. Geneste and Herscher's opinion, it was always desirable to aim at a process which would consist in compensating the loss of heat through walls and windows by local radiators placed in the apartments, in such a way that the air introduced for ventilation should not need to be heated above the degree suitable for respiration. Had the matters been referred to in the Author's Paper, they would have had something to say on the disposition of the air-supply, and of the heated-air inlets into apartments.

In conclusion, while acknowledging the thoroughly practical character of the Author's Paper, they deprecated the far too secondary position to which he had relegated the system of ventilation with warmed air, combined with local heat-radiators, which they considered to be the one most conducive to comfort and to economy. They always employed that system themselves where possible, and, except in the cases of public halls, theatres, and such buildings, were always careful to place the local radiators in contiguity with the coldest wall.

But they must add that they passed over these surfaces a

Messrs. Geneste and Herscher.

certain volume of air taken directly from outside, that was to say having only to traverse the thickness of the wall; this amount o air being calculated so as to produce a convenient ventilation in proportion to the use to which the apartments were put, and the number of their occupants. The amount of heating-surface was itself calculated so as to impart to that air the temperature most convenient for respiration, and to radiate into the apartment air at the desired temperature. That temperature could be maintained as high as was desirable in a habitation. They considered that it was not well to give the air, when introduced, a temperature too high, for in that case it tended to ascend at once from the radiators to a height above that where it was needed for respiration, that was to say, they preferred that the air should not at once reach the upper parts of the room, to descend when it had been mixed with the vitiated stratum at the top. The practical means of meeting this difficulty was to divide the radiator by a partition, so that one part was devoted to warming the fresh air introduced, while the other radiated the heat into the apartment.

The disposition of the heating-surface along and below the coldest partitions, and notably the windows, had the effect of preventing descending currents of cold air, which always existed in contiguity with cold partitions. These currents were not only a source of discomfort to individuals, but interfered with the proper circulation of the air by promoting a flow of vitiated air in their neighbourhood.

It was, of course, understood that a general system of ventilation must provide for the outlet of the vitiated air, which was effected by partial openings communicating with one or more collecting-conduits having access to exhausting-chimneys, in which it might be desirable to place a heating surface or a gas-burner, in order to promote an upward draught.

In public halls or apartments of some extent, destined to contain a large number of persons, they effected ventilation by a blower causing the air to pass over radiators; but even in such cases they employed, if possible, local radiators as well. They entirely approved of the Author's system of reheating the air by passing it through an auxiliary coil before letting it circulate in the airways. It would be conceived that this mode of heating by local radiators involved a larger division of the steam-pipes; it was therefore all the more important that special precautions should be taken to avoid escapes of steam or irritating noises. It was also necessary to assure for this disposition all the advantages it offered of ren-

Messrs. Geneste and Herscher.

dering the different heating-surfaces independent of one another, so that the temperature of any apartment could be modified at will. The system of distributing the steam they had described perfectly fulfilled these conditions, and gave the best results.

Mr. Hett.

Mr. C. L. HETT observed, in reference to the description by Mr. Rigg of the heating of a printing-office by the exhaust steam from the engine, where the capacity of the room was equal to 200 cubic feet for each lineal foot of 4-inch heating-pipe used, that two of his workshops were heated in a similar manner. The first, which was rather cool, had 350 cubic feet for each foot of 4-inch pipe. The second, where the air was maintained at a pleasant temperature, had 466 cubic feet to each foot of 4-inch pipe. The above variation might be accounted for thus:—In the first-mentioned shop, the window-area was large, and the glass thin. The doorway area was also large, the doors being at each end of the shop. In the second shop the window-area was small, the glass thick, and a door, small compared with the size of the building, was placed in the centre of one of its sides. A 2-HP. engine also worked in this shop. The first of these shops was heated in the year 1878, after he had seen the plan in use in Leeds; where, he believed, it was generally adopted in engineers' workshops.

The question as to the quantity of steam passing through the pipes was one which did not appear to him to affect the heating efficiency of the pipes, provided that the whole of the steam was not condensed. With regard to the high-pressure system, he had recently heated the reading and billiard rooms of a club. In each room there was a fire-place. External air was admitted to the pipes in the billiard-room, while the reading-room was without any arrangement for that purpose. The result was, that the billiard-room was perfectly sweet, while the reading-room smelt close and unpleasant. It should, however, be mentioned, that efficient roof ventilation was provided in the billiard-room.

Mr. Hood.

Mr. C. HOOD observed that all that was mentioned in the Paper as being new in America had been both fuller and better stated by Buchanan,[1] and by Tredgold in three editions of his treatise on steam-heating, 1820, 1824, and 1836.[2] The processes described by the Author had been practised extensively for the last fifty or

---

[1] "Essay on the Warming of Mills and other Buildings by Steam." By Robertson Buchanan, 1807. "A treatise on the economy of fuel, and management of heat, especially as it relates to heating and drying by means of steam." By Robertson Buchanan. 8vo. Glasgow, 1815.

[2] "Principles of Warming and Ventilating Public Buildings," &c. By Thomas Tredgold. 8vo. London.

Mr. Hood. sixty years in England, to Mr. Hood's personal knowledge. Steam-heating on a very large scale had been successfully used for several years by Loddiges of Hackney, commencing in the year 1819, and had never ceased to be used since that time, precisely as the Author described it as a new invention in America.

General Meigs. General M. C. MEIGS, U.S.A., observed that the Author of the Paper and his employer, Mr. Nason, had done much to settle the American practice of warming buildings. They had heated a large number of lunatic asylums, which were among the most extensive buildings in America. They had built, between 1856 and 1861, the heating apparatus of the wings of the Capitol of the United States in Washington. The apparatus then put into the north wing, occupied by the Senate, was still in use; that put into the south wing was also in use; but the great heating-coil of the Hall of Representatives, containing 10 miles of wrought-iron pipe, had been moved to a more central position. It was while engaged upon the studies for this work that the Author conducted for General Meigs a course of experiments upon fans, which fixed the proportions of fans for forced ventilation as now very generally used in the United States.

He devised the method of forcing a regular circulation of steam in small vertical pipe-coils by using a thin strip of hoop-iron inserted longitudinally, which practically divided the cylindrical 1-inch pipe into two pipes of semicircular section. This economical arrangement was a great success, but the rusting out of this thin hoop-iron in presence of "air and condensing steam" proved objectionable, and the vertical iron pipe, cast in couples or loops (Bundy's radiator), was the one now mostly adopted.

The new National Museum building in Washington was warmed by these radiators placed around the base of the walls of the halls into which it was divided. The building was 314 feet square on plan. Its capacity was 3,534,000 cubic feet in one great hall, which was divided by rectangular piers into naves, transepts and galleries, all, however, communicating by lofty and wide open arches, which connected the tops of the piers and supported the roof. The condition of the art of steam-heating at this time was illustrated by the process successfully adopted for procuring the heating apparatus of this building. On the 31st of October, 1879, the Building Commissioners published an advertisement asking tenders for heating the building. The specifications and plans gave the dimensions of the structure. The apparatus was to be for low-pressure steam; the building to be heated to 72° Fahrenheit, the halls to 68°, when the external temperature was at zero. The maximum pressure of steam was not to exceed 5 lbs. on the square inch above atmospheric pressure, and perfect circulation through the whole apparatus at 1 lb. pressure was to be guaranteed. General Meigs.

Nine firms tendered at prices ranging between $55,680 and $13,940. A bid at $19,768 was accepted, and the whole work was completed and in action at this cost on the 9th of December, 1880. Several of the firms bidding offered to complete the whole work in three months. There were four steam-boilers in a vault below the south-west corner of the building, each 56 inches in diameter, 15 feet in length, and having seventy-two tubes 3 inches in diameter. The heating surface of the boiler was 3,836 square feet; the nominal HP. 256. Two main-supply steam-pipes were 8 inches in diameter; the total radiating surface of the steam-coils was 13,680 square feet. The cost of this heating apparatus amounted to about ¼*d.* per cubic foot of space heated. It had been perfectly satisfactory and very economical. The grand hall, 300 feet square on plan, was kept at 75° with an outside temperature of 12°. This heating was by direct radiation, the cheapest and generally the most comfortable. Visitors coming in from the cold outside found it pleasanter to stand by or over a heated coil, to thoroughly warm themselves at once, than to wait for the slow process of dispelling the chill of garments and body by contact with air at 70° or 75°. In public halls like this it was well to have registers in the floor; admitting heated air from coils below, or some flat plates of iron heated by steam upon which persons entering from outside and chilled by the cold might stand till their shoes and feet were dried and warmed.

General Meigs was now erecting a large building for the United States Pension Office, 400 feet by 200 feet, three stories high, surrounding a lofty central hall 316 feet by 116 feet. He contemplated heating this building in the same way by tubular boilers in vaults under the south-west corner, using Bundy's direct radiators around the hall and under the exterior windows of the clerks' rooms. It was intended to accommodate fifteen hundred clerks at their desks. The use of radiators under windows supplied with air directly through openings in the breast of the window had not proved a success. The circulation of steam was not always rapid enough to prevent congelation by currents of air striking the pipes at temperatures below zero. But when the air was not allowed to strike the pipes directly, the window-breast appeared to be the best position for such radiators as were now in general use.

The hot air issuing from the radiators met and corrected the

General Meigs. excessive coldness of the sheets of air descending in contact with the inside of the window. He found a considerable economy of heat in the use of double glazed windows; not two sets of distinct sashes, but of two panes of glass in the single sash. The intervals between the glasses should not be less than 1 inch, which required a sash 2 inches in thickness. The National Museum was glazed with double panes having an interval of $\frac{3}{4}$ inch, although 1 inch was better, and was quite sufficient. A thermometer placed in the interval between the glasses in a north window, constantly indicated a temperature within 0°·5 Fahrenheit between the mean temperature of the exernal air and of that inside the room. In a south window, or one exposed to direct sunlight, it was always higher than the outside air, some 30° or 40° higher in the direct sun's rays, and higher in the shade. This last was due to radiation from the walls and the ground heated by the sun, even when partially obscured by clouds. With an external temperature of 0° and an internal one of 70° therefore, the apparatus was required to heat the air in the room against a temperature of 35° in the window, instead of against one of 0° outside, which would be its task with a single thickness of glass in the sash.

With the inventions and contrivances of Mr. Nason, the art of steam-heating was placed in America upon a basis which had enabled many persons to undertake, at fixed prices, any desired work in heating buildings large or small. Great progress was being made in this respect by steam generated at large central boiler depots. Lockport, in the state of New York, and Detroit, in Michigan, were among the considerable towns in which the system was in operation; and at the present time the streets of New York were torn up by companies laying down steam-supply pipes, and many houses, stores, shops and some hotels depended upon the steam thus furnished for heating, for cooking, and for power.

The use of scales, such as that shown by Fig. 2, was due to the Author. His practice in the workshop was to have these scales drawn with a scratch-awl or hard point on plates of copper or brass. The dimensions for a fitting of any desired diameter could thus be taken at once from the scale by the dividers, without tedious calculation, and without making new working drawings.

Mr. C. A. Smith. Mr. CHARLES A. SMITH, of St. Louis, U.S.A., remarked that in the Appendix the Author had taken as the first of his nine data the equivalent of a rate of transfer of $1\frac{3}{10}$ heat-unit per hour per square foot of iron radiating surface per degree difference of temperature between the steam and air. But his experience was that this depended wholly upon the circulation of the air, and that a

Mr. Smith.

rate of 2 $\frac{6}{10}$ units in place of 1$\frac{3}{10}$ could easily be secured. As Mr. Briggs had taken the temperature of the warm air leaving the radiators, while he used the average temperature of the room there was not such a difference in the extent of surface required. The amount of heat needed to maintain a uniform temperature in a building was dependent only upon the quantity carried through the external surface, walls, windows and roof, which again depended on the difference of temperatures between the air inside and outside of the building, and the circulation of air outside. He had found for ordinary buildings, including ample ventilation, that during high winds a rate of 1$\frac{25}{100}$ unit per square foot of external surface per hour per degree difference of temperature was reached; while usually it was about half that rate. With better class buildings, having thicker walls and air-spaces, a lesser rate was obtained.

5 December, 1882.

Sir W. G. ARMSTRONG, C.B., F.R.S., President,
in the Chair.

The following Associate Members have been transferred by the Council to the Class of

Members.

Cecil William Edward Henslowe.
Henry Rigg.
William Hanbury Pettingal Sherman.
George Eastlake Thoms.
Reginald Foster Ward.
John Hatton Wilson.

The following Candidates have been admitted by the Council as

Students.

Walter Allen.
James Henry Anderson.
Walter Duncan Barrow.
Norris Garrett Bell.
Louis Herbert Butcher.
Ernest Romaine Callender.
Edmund Charrington.
Edward George Clark.
Stephen Butter Cottrell.
William Robson Crabtree.
David George Phillips Davies.
William Dawson.
Lewis Marie Theodule Devéria.
Francis Charles Dixon.
Hugh Copner Wynne Edwards.
Robert David Fitz-Gerald.
George Wadham Floyer.
John William Gardner.
Alfred John Gillingham.
John Gladden.
Robert Maynard Gloyne.
Horace Mark Gregory.
William Wylie Grierson.
Frank Herbert Hebblethwaite.
Arthur Ernest Hezlet.
George William Higham.
George William Holmes.
William Wykeham Jacomb.
William Alfred Jenkin.
William Hemming Jones.
Riyosaku Kuri.
Frederic Nix Latham.
Frank Robert Leith.
Frederic William Macaulay.
Stephen Martin-Leake.
Warine Ben Hay Martindale.
George Vincent Maxted.
Charles Erskine May.
Richard Moreland (*tertius*).
George Thomas Murray.
Frank Crumpton Nunn.
John Frederick O'Connor.
Adolphe Ernest Orr.
Francis Adolphus Pawley.
Henry Etienne Pellereau.
Oswald Pennington.
Reginald Seymour Prinsep.
William Joseph Reilly.
George Richard Richardson.
John Arthur Saner.
Percy Edward Scrutton.
George Herbert Shaw.
Ernest Headly Sprague.
Ernest Albert Strickland.
Percy Crosland Tempest.
Edwin Bailey Thomas.
George William Thompson.
Edgar Charles Thrupp.
Valery Theobald Guillaume de Vismes de Ponthieu.
Thomas Robert John Ward.
Walter James Weightman.
Charles Arthur White.
Seymour William Price Williams.

The following Candidates were balloted for and duly elected as:

Members.

John George Henry Collister.
James Edmund Fitzgerald Coyle.
Edward Bayzand Ellington.
Anthony George Lyster.
John Edward Mammatt.
Edward Emmerson Oliver.
John Price.
José Americo dos Santos.
George E Waring, Jun.

Associate Members.

David Angus.
Edward Drake Baring-Gould.
William Townshend Batten, Stud. Inst. C.E.
Ranson Colecome Batterbee.
William Reid Bell.
John Richard Boger.
Alexander Cunningham Boothby.
George Cecil Herbert Brown, Stud. Inst. C.E.
Alexander Fairlie Bruce.
Andrew Duncan Cairns.
Marcelin John Chabrel.
Timothy Augustine Coghlan.
Edward George Cronin.
John George Davidson.
Charles Abraham D'Ebro.
Paul Ewens.
Edward Brocklehurst Fielden, Stud. Inst. C.E.
Edward William Perceval Foster.
Percival Fowler, Stud. Inst. C.E.
Robert James Frecheville.
Henry Francis Burnes Frost, Stud. Inst. C.E.
William Hayden Gates, Stud. Inst. C.E.
Duncan George, Stud. Inst. C.E.
George Gregory.
Alberto Joseph Pimentel Hargreaves.
Francis Bradstreet Heaphy.
Charles Hopkinson.
Andrew John Hudleston, Stud. Inst. C.E.
Joseph William Jenkinson.
Alexander Anderson Kyd, Stud. Inst. C.E.
Samuel Petty Leather.
Robert Charles Longridge.
Ernest du Bois Lukis.
Isaac Shepherd McKie.
Kiyoshi Minami.
Radhica Prosand Mookerjee.
Walter Campbell de Morgan.
Louis Neville.
Alfred Cornelius Newcombe.
William Edward Newham.
George Victor Pont.
Richard Popkiss.
Edward Quick, Stud. Inst. C.E.
Howard Rich.
John Newman Robinson, Stud. Inst. C.E.
David James Ross.
Ernest Samuelson.
James Charles Shears, Stud. Inst. C.E.
Walter Shellshear, Stud. Inst. C.E.
Edmund Caswell Bowyer Smijth.
Edward Felix Stephens.
Thomas William Stone.
John O'Brien Tandy.
Emilio Mitre y Vedia.
Henry Wesley Voysey, Stud. Inst. C.E.
Thomas Alexander Wade.
Henry Edward Weaver.
William Butler Whitaker.
George Whitehouse, Stud. Inst. C.E.
Thomas Brown Younger.

Associates.

Frederick Attock.
Allan Joseph Champneys Cunningham, *Major*, R.E.
Farnham Maxwell Lyte.
Henry Coddington Meyer.
Henry Morland.
John Eardley Yerburgh.

(*Paper No. 1866.*)

"On the Sinking of two Shafts at Marsden, for the Whitburn Coal Company."

By John Daglish, M. Inst. C.E.

It has long been known that the North of England coal-field extends under the sea on the coasts of Northumberland and Durham; but although the coal had been worked up to the coast line at several points, many years ago, no operations had been carried on under the sea on the east coast of England, until within the last few years.

Recently, however, leases of the under-sea coal have been negotiated by various large mining companies from the Crown, along nearly the whole coast line from Newbiggen on the north, to Castle Eden on the South (Plate 1, Fig. 1). And at several places (Seaham and Ryhope to the south, and Monkwearmouth to the north of Sunderland, and North Seaton to the north of Blyth) where the existing pits were comparatively near the coast, the workings have now been extended to a considerable distance under the sea. At other points, however, the coal-field under the sea cannot be reached by any existing pits, and special pits will be necessary for working these portions.

An important geological feature of this district is the outcrop of the Permian rocks about midway on the coast line of this coal-field, at the mouth of the River Tyne; the Coal-Measures, which appear on the surface between the River Coquet, at the northern extremity of the coal-fields, and this point, here dip under, and are overlaid by the Magnesian Limestone as far as the southern extremity of the field at Castle Eden, near which point the coal-field is again overlaid by the red marl of the Trias formation. The dip of the magnesian beds being southerly, they gradually increase in this direction, from a thickness of a few feet under the Priory at Tynemouth to upwards of 600 feet at Castle Eden (Plate 1, Figs. 1, 2, and 3). Underlying the Magnesian Limestone and stratified conformably with it, and unconformably with the Coal-Measures, is a bed of sand varying in thickness from a few inches to 100 feet; and in quality from hard rock to almost incoherent sand. This bed is well developed under the Priory Rock at Tynemouth (Fig. 2), and in the railway cutting at Ferryhill. Where this bed is of great thickness and of incoherent character, it has been the cause of a large expenditure of capital and time, at many of the colliery sinkings in the south east part of the Durham coal-field, and notably at the Murton Colliery.[1]

A considerable area of submarine coal, extending for a distance about 3 miles along the coast, intermediate between the towns of South Shields and Sunderland, was acquired by the Whitburn Coal Company in 1873, together with a portion of the land coal adjoining. To win this area it was decided to sink two or more shafts near Marsden, at a distance of 500 yards south of the Souter Point Lighthouse and 400 yards from the sea. As these shafts had to pass through a considerable and unknown thickness of Limestone, which always contains large quantities of water, and as the thickness and character of the underlying Yellow Sand were also unknown, it was from the outset anticipated that difficulty would be encountered in sinking them to the Coal-Measures, especially in passing through the Sand. A preliminary boring was made, by the ordinary process, which proved the thickness of the Limestone to be 340 feet, and fortunately the entire absence of the Yellow Sand at this point.

The same general arrangements which a few years before had been successfully carried out by the Author at a similar sinking at the adjacent Silksworth Colliery, the property of the Marquis of Londonderry, were followed at Marsden. In the first instance an engine (A, Fig. 5), with two cylinders each 48 inches in diameter, capable of being employed as a pumping engine during sinking, and afterwards to be used for winding or drawing coals, was erected at No. 1 pit. This engine is a duplicate of the one erected at the Silksworth Colliery,[2] and that erected more recently at the adjoining Boldon Colliery. A second engine, B, having two cylinders, each 44 inches in diameter, hereafter to be used for driving the ventilating machine, was also arranged for pumping out of No. 1 pit; and a third and smaller engine (C), with two cylinders each 26 inches in diameter, for pumping out of No. 2 pit. This was intended to be used as a temporary coal-drawing engine, and ultimately as an underground hauling engine.

The sinking was commenced on the 23rd of December, 1874, and at a depth of 35 yards the water-bearing stratum was reached, 2 feet below high-water mark of ordinary spring-tides. After

---

[1] North of England Institute of Mining Engineers. Transactions (1856-7), vol. v., p. 43.

[2] North of England Institute of Mining and Mechanical Engineers. Transactions, vol. xxv., p. 201, and vol. xxix., p. 3. Bulletin de la Société de l'Industrie Minérale. 2ᵉ série. Tome vi. 1877, 2ᵉ partie, p. 411.

sinking a few feet further, a large feeder, or spring of water was met with, and the first set of pumps was put in. With these engines 3,200 gallons of water were pumped per minute; but it was soon found that the water could not be successfully kept under by these appliances. The engines having still a large surplus of power, it was decided to procure extra and larger pumps, to endeavour to overcome the water by pumping in the ordinary way. The arrangements then carried out consisted of the following, viz.:—

No. 1 *Pit.*—Two 30-inch sinking sets, with 6 feet length of stroke, were attached to one end of a double-ended quadrant, and worked by the large engine (A). These lifted to two similar sets (in a staple) attached to the other end of the quadrant. One 20-inch sinking set, with 5 feet length of stroke, lifting to another, was attached to the second engine (B).

No. 2 *Pit.*—Two 20-inch sinking sets, with 5 feet length of stroke, lifting to the surface, were worked by the engine (C).

With the assistance of these pumps, the pits were sunk to a depth of 51 yards from the surface, or 16 yards below the level of saturation, when it was found impossible to continue the sinking further against the enormous quantity of water. By this time the water had become strongly saline, and it was clear that an influx had set in from the sea, through the open gullets in the limestone, and that sea-water was being pumped.

It was then decided to discontinue the sinking for a month, and to endeavour to drain the district of the land waters by steady pumping. The engines were accordingly set to work at the following speeds:—

Engine.	Strokes per Minute.	Pumps.		Quantity of Water Pumped in Gallons per Minute.[1]
		Number of.	Diameter.	
			Inches.	
A	17	1	30	6,120
		1	30	
B	26	1	20	1,820
C	27	2	20	3,672
				11,612

This quantity of water was pumped for a month without accident; and indeed during the whole of the period of four months of pumping no accident beyond the breaking of two Spears occurred. The above is probably the largest quantity of water

---

[1] At one time the rate was somewhat faster, and a total quantity (by calculation) of more than 12,000 gallons per minute was pumped.

ever pumped at one mine, and also the highest speed at which such large pumps have been worked.

This enormous drainage-power succeeded in overcoming the water sufficiently to enable the sinkers to resume operations in the pit; but the influx of water was so rapid that it was evident the cost of sinking in the ordinary way would be too great to be continued. The influx of water into the shaft when the pumping stopped was at the rate of 12 feet in height in the shaft in two minutes.

The arrangements of the various engines and crabs on the surface are shown in Plate 1, Fig. 5.

It was then decided to adopt the Kind-Chaudron process for the further prosecution of this undertaking, the Author having given considerable attention to this method prior to the sinking of the Silksworth colliery. In that case, however, as the quantity of water met with in sinking through the Magnesian Limestone never exceeded 1,000 gallons per minute, it was overcome by a single 20-inch set of pumps; and therefore that sinking was more rapidly and economically completed by the ordinary process.

KIND-CHAUDRON PROCESS.

On the 2nd of May, 1877, steps were taken to remove the head-gear at No. 1 shaft (which had been erected and used for the ordinary sinking), as it was intended to utilise this shaft, so far as it was already sunk, and to continue the sinking of a somewhat smaller pit within by the Kind-Chaudron process. The diameter of 14 feet 3 inches was ultimately chosen for this shaft, on account of its having been the largest size hitherto sunk by this system on the Continent. The whole of the tools were purchased secondhand, and had been used and tested in previous sinkings. This not only considerably reduced the cost, but also eliminated the risks that attend the use of new tools.

The first operation was the lowering of a wrought-iron tube, ½ inch thick, and riveted with countersunk rivets so as to form a smooth surface, 54 feet long and 14 feet 4 inches in internal diameter, from the top of the water to the bottom of the shaft, so as to ensure that no stones could fall from the sides during the boring. This was safely accomplished on the 16th of August, 1877, and the space between the tube and the sides of the shaft were then filled in with concrete, and the boring commenced by the Kind-Chaudron process at a depth of 155 feet, on the 24th of September, 1877.

As this process of boring out pits has been fully described in Papers read before the North of England Institute of Mining and Mechanical Engineers,[1] in May 1871, by Mr. Warington W. Smyth, and before this Institution, in March 1872, by Mr. Emerson Bainbridge, Assoc. M. Inst. C.E.,[2] the Author will only very briefly allude to the tools employed, and give in greater detail the particulars wherein the operations at Marsden differed from those previously described.

DESCRIPTION OF TOOLS, &c.

A substantial Headgear was erected, strongly framed together with timbers (Plate 2, Figs. 1 and 2). The whole of this is covered in with wood cleading, so that the workmen are always protected from the weather. At 37 feet from the ground, two rails are laid on stout balks A A of timber, which carry travelling carriages X X, on which the heavy tools are run backwards and forwards. At 52 feet from the ground similar rails, on longitudinal balks of timber B B, support small carriages (Plate 3, Fig. 8) for carrying the boring-rods, this great height being necessary in order to obtain sufficient length of rods. It is this system of carrying and moving the tools on traversing carriages which enables the operation to be conducted with so small an amount of manual labour.

DESCRIPTION OF THE PROCESS.

The Kind-Chaudron process consists of two distinct series of operations.

1st. Those connected with the boring out of the shaft, on a system closely resembling that first adopted by Mr. Kind many years ago for boring deep holes for artesian wells. 2nd. That of lowering down the shaft a water-tight lining or Tubbing.

The first process therefore at Marsden was the boring of a centre hole in No. 1 pit, 4 feet 11 inches in diameter, by a small Trepan or chisel (Plate 3, Fig. 1). This Trepan, 7 tons in weight, is attached to the massive wooden Lever (A, Plate 2, Figs. 2 and 3) by rods of the best pitch pine, 5 inches square (A, Plate 3, Fig. 4), and 58 feet long, with iron terminations, having tapered screws. One end of each rod is fitted with a male screw (A, Plate 3, Fig. 5), and the other with a female screw. The screws have coarse

---

[1] Transactions, vol. xx., p. 187.

[2] Minutes of Proceedings Inst. C.E., vol. xxxiv., p. 43.

threads carefully cut, so that, after having entered, a few turns are sufficient to screw the joint quickly home.

The Lever is attached on the opposite end to a steam cylinder (C, Plate 2, Figs. 2 and 3), 39 inches in diameter, actuated by a single valve only on the top side. The valve is worked by hand, the rods are lifted by the pressure of the steam on the top side of the piston, and they fall by their own weight when the valve is opened to the atmosphere. The length of stroke is regulated by the machinist, and varies from 6 to 18 inches according to the hardness of the rock. An important adjunct to the Lever is the Spring Beam (B), against which the Lever strikes at the termination of each stroke. The number of strokes per minute varies from nine to eighteen. In very hard rock comparatively few and light blows only can be given. When the rods are suspended at the end of each stroke, they are turned through an angle of 2° to 4° by four workmen holding a crosshead lever, walking round the top of the pit, similarly to an ordinary boring.

An essential part of the boring tools is the Sliding Piece (Plate 3, A, Figs. 1 and 3), by which the Trepan is connected to the rods through the medium of a slot 12 inches long. This permits the Trepan to strike the bottom without communicating a severe shock to the rods, which continue their descent until arrested by their buoyancy in the water, aided by the Spring Beam striking against the inner end of the Lever. Except for the play thus allowed, it would be impossible to strike even a light blow without fracturing the rods.

An apparatus called the Freefall (Plate 3, Fig. 2) is sometimes also attached. On the descent of the rods the Trepan is caught up by a pair of jaws (A), which are locked by a wedge. The wedge being withdrawn by means of a large disk of wood (B) at the commencement of the return stroke, permits the Trepan to fall nearly 2 feet without being detached from the rods. This apparatus was attached to the small Trepan in boring the No. 2 small pit between the depths of 284 feet and 334 feet. A disk, 5 feet 2½ inches in diameter, gave most satisfaction, the diameter of the small pit being 6 feet 6¾ inches.

After the boring has been continued about three hours, in moderately hard rock, the Trepan is withdrawn, and the Sludger (Plate 3, Figs. 6 and 7), with a capacity of 4 cubic yards, or 10 tons, is lowered. The Sludger is sometimes attached to the Lever, and worked up and down by the rods, and at other times by the rope only. The *débris* rises into it through the valves in the bottom, it is then withdrawn and emptied. The emptying of the Sludger, and the unshipping of the Lever, to allow of the rods being removed, are effected by ingenious and time-saving arrangements, which must be seen to be understood.

After the centre-boring is advanced 30 or 40 feet, the large Trepan (Plate 3, Fig. 3), 16 tons in weight, is put in, and the large pit is similarly bored, the *débris* falling into the small pit, which requires to be frequently cleared out. This was the process in the first instance adopted at Marsden, but it was afterwards modified. In every new sinking by this system slight variations are found in the character of the rock, which entail modifications in its application. At Marsden the rock proved to be harder than in any locality where the system had been previously in operation.

During the boring out of No. 1 small pit no difficulty was found in raising the *débris* with the ordinary Sludger; but in boring the large pit it would not rise into the Sludger, but became solidified at the bottom of the small pit. This was probably due to the particles being larger than those produced in the boring of the small pit. To remedy this, at first clay was thrown down the pit, and the small Trepan was again introduced to loosen the *débris*, and mix it with the clay, which could then be withdrawn by the ordinary Sludger. But the process was a long one, the re-boring taking quite as much time as the original boring. It was therefore determined to lower the Sludger into the small pit, release the rods, and leave it there to catch the *débris* as it fell. Accordingly the Sludger was lowered to the bottom of the small pit, and left there, as shown in Plate 4, Fig. 1. On attempting to withdraw it, however, it was found that the mud which had settled in the water at the bottom of the pit, or which had passed the sides of the Sludger, embedded it so far that great violence had to be used to extract it, which would have certainly, sooner or later, resulted in serious accidents. Arrangements were then made to suspend the Sludger on the edge of the small pit at the top by Claws (Plate 4, Fig. 2), and the two inner of the interior teeth of the large Trepan were removed to avoid striking these claws. This plan succeeded imperfectly, and on several occasions when the Claws were struck the Sludger fell down the small shaft, and was only extracted with difficulty, and with a liability to accidents.

A successful attempt was then made to form a ledge within the smaller pit, by taking out all the teeth but the two outer, and the Sludger was thus suspended about 1 foot from the top of the small pit. This operation, however, entailed so many changes of the teeth, &c., that it was attended with great loss of time. But having found the correct principles on which to proceed, it was not difficult to devise a plan for leaving a suitable ledge within the smaller pit. To effect this, the outside tooth of the small Trepan on each side was enlarged 3 inches; the tool was again introduced, and the small pit bored to a diameter 6 inches wider than previously, leaving a ledge of 3 inches all round (Plate 4, Fig. 3), on which the Sludger was suspended by an angle-iron ring.

In No. 2 pit a third Trepan was used, having a diameter of 6 feet 6 inches. By this Trepan the small shaft was bored to a depth of 383 feet; not only through the Limestone, but 50 feet into the Coal-Measures, and 6 feet 6 inches below where it was intended to place the Moss-box of the Tubbing, and therefore below, and entirely clear of, all future operations with the large Trepan.

The smallest Trepan was then introduced, and the boring continued 32 feet 9 inches further, leaving a ledge of stone 9½ inches in width all round; on this ledge a cast-iron ring was deposited, to form a permanent bed for the Hanging Sludger to rest on. This arrangement acted perfectly, never having been the cause of the slightest accident throughout the sinking of the second shaft.

The cast-iron ring was adopted in the second pit, because the weight of the Sludger soon wore away the ledge of stone by being suspended from it. At first the Hanging Sludger was lowered into its seat by the regular screw, which was left slightly slack, all the other screws of the rods, as they were lowered in, being tightly screwed home. When the Sludger was deposited on its bed, by turning the rods backwards, the slack joint yielded, and the rods were unscrewed at this point and drawn away. It did, however, happen occasionally that some of the other screws became detached, and then the remaining rods and sludger had to be fished up. A double hook (Plate 3, Fig. 11) was next adopted for lowering the Hanging Sludger into place; it was simply fastened on to the bow of the Sludger, and when the latter was lowered and rested on its bed, the rods were let down a few inches further, and turned half round, so as to free the hook entirely from the bow; they were then drawn away, leaving the Sludger in place.[1]

The Author has described this portion of the operations in detail, being the first instance in which the Hanging Sludger was used

---

[1] The bottom of the rods where they are attached to the Sludger by a female screw is fitted with a small inverted funnel (Plate 4, Fig. 5), to guide the male screw which is attached to the Sludger, into the female screw at the end of the rods, as they are lowered; an arrangement successfully carried out through the whole of the boring of both pits, without failure or difficulty, even at a depth of nearly 400 feet.

in the Kind-Chaudron process for shaft boring, although it had been used by Mr. Kind, on a somewhat similar design in boreholes.

SAFETY TOOLS.

No small part of the success of this process arises from the ingenious arrangements, and forms of tools, for picking up material at the bottom of the shafts, and for taking hold of broken spears, &c., which, from the character of the operations, must be of frequent occurrence. These are termed "safety tools," and consist of the following apparatus:—

1st. The Catching Hook (Plate 3, Fig. 12), which, on being swept round the shaft below the top of the broken spear, guides the spear into the angle made by the hook and its rod, where a properly-shaped recess is formed, into which the ironwork of the spear falls, and can by this means be retained and withdrawn.

2nd. The Spear Catcher (Plate 3, Fig. 13) is a fish-head, with a pair of serrated jaws, which on touching the top of the broken rod, and the wooden chock keeping the jaws open being forced out, the teeth press firmly against the ironwork of the Spears, enabling them to be withdrawn.

3rd. The Grappling Tongs (Plate 3, Fig. 14) being a pair of large rakes, which can be opened and shut by levers worked by ropes. By moving and working this across the bottom of the shaft, any pieces of material larger than 2 inches square, can be extracted with ease.

THE TUBBING.

The most important part of the process, and that attended with the greatest risk, is that of lowering into the shaft the metal Tubbing. At the Marsden Sinking, the dimensions of each ring or cylinder were as follows (Plate 4, Figs. 6 and 7):—

——	No. 1 Pit.			No. 2 Pit.		
	Feet	Inches.		Feet	Inches.	
Internal diameter	12	7½		13	8½	
External ,,	12	9⅛		13	11¾	
Thickness of top cylinder	0	1		0	1⅝	
,, ,, bottom ,,	0	1⅝		0	1⅞	
Height of each cylinder	5	0		5	0	
Total height of Tubbing	280	0		285	0	
	Tons	cwt.	qrs.	Tons	cwt.	qrs.
Weight of top cylinder	5	4	0	6	10	1
,, ,, bottom ,,	7	0	0	8	19	2
Total weight, including bolts and lead joints	400	0	0	450	0	0

The flanges of each top cylinder are 3½ inches wide by 2 inches in thickness; and between every ring is placed a plain lead wedge 4⅞ inches wide, by ⅛ inch thick, covered on each side with red lead. The cylinders are attached to each other by sixty $1\frac{3}{16}$-inch bolts of best iron. The whole of these cylinders are alike, save in varying thicknesses, excepting the bottom three pieces. The bottom pieces A and B are telescopic, with outside flanges C and D, 6 inches and 7⅝ inches, respectively; the bottom piece, B, was suspended to the upper piece by rods in No. 1 pit, and in No. 2 pit, by an internal flange, which permits of the second piece (A) sliding downwards on the outside of the first piece. Whilst being lowered, the outside flanges of the bottom pieces, which are called the Moss-Box (and which are the only two cylinders with outside flanges), are 5 feet apart, and the interval is filled with tightly compressed moss. When the lowest piece rests on its bed, at the bottom of the pit, the remainder of the cylinders continue to descend, compressing the moss with the whole weight of the Tubbing, namely, over 400 tons.

In the middle of the third cylinder from the bottom there is an extra internal flange E, 3½ inches wide; on which is screwed, by sixty-four bolts, a flat ring or circle of cast iron F, 5¾ inches broad. This ring admits of the False-Bottom G being withdrawn up the interior of the Tubbing to the surface, when the operation of lowering the Tubbing has been completed. A massive dish-plate G, of cast metal 1½ inch thick, is bolted to the bottom, having a flange H on the upper side, for attaching the column of pipes. The object of the False Bottom is to float the Tubbing whilst it is being lowered.

After carefully securing together by their respective flanges and attachments the three pieces of Tubbing intended for the bottom, they are lowered to the level of the water by an arrangement of screw-rods worked by six powerful winches, with two men to each; additional cylinders and central pipes are then added one by one, causing the whole of the Tubbing to sink until it floats by the displacement of the water. In the Marsden No. 2 pit the Tubbing floated when cylinder No. 9 was attached. The rods are thereupon removed, and as each additional cylinder is added, a certain quantity of water is run inside to cause the Tubbing to sink. In the Marsden No. 2 pit the addition of cylinder No. 10 caused the Tubbing to sink 1 foot 9 inches, and of cylinder No. 56 at the top 1 foot 1 inch.

In both pits this operation was completed without leakage, either at the joints of the cylinders, or of the central column of pipes. The work, however, requires great care and watchfulness, being attended with risk, as any leakage would cause the Tubbing to sink to the bottom.[1]

CONCRETING.

This operation consists in filling with concrete the annular space between the exterior surface of the Tubbing and the sides of the shaft, from the Moss-Box upwards to the top of the Tubbing (I, Plate 4, Figs. 6 and 7). The concrete is lowered simultaneously all round the pit by four rectangular boxes, 3 feet long, 18 inches broad, and 4½ inches wide, shaped to the radius of the pit (Plate 3, Fig. 15).

A large gullet was passed through in No. 2 Pit at a depth of 56 yards from the surface, the width of which was nearly the whole diameter of the shaft. When concreting at this point, 120 cubic yards of small stones and concrete were filled in, and 80 and 40 cubic yards at smaller gullets lower down (Plate 4, Fig. 8), without sensibly raising the level of the concrete.

ACCIDENTS.

The only accident that occurred during the execution of the works at Marsden of special interest was the loss of one of the teeth of the small Trepan in the No. 1 Pit. The difficulty attending this accident arose from the tooth having been deeply embedded in the rock at the bottom of the borehole by repeated blows of the Trepan, before its loss was discovered, after having fallen from its socket. Upon withdrawing the Trepan the Grappling Tongs were introduced, and the position of the tooth accurately determined, but so firmly was it embedded that the Tongs were unable to raise the tooth. It having been thus ascertained that the embedded tooth was at the edge of the pit, in the position occupied by the teeth at the extreme edge of the Trepan, these end teeth were removed, and the Trepan lowered again, and boring recommenced and completed to the

---

[1] In the deep sinking at Ghlin near Mons, now in operation, the depth bored is 1,026 feet, with an internal diameter of 14¾ feet. The thickness of the Tubbing at the top being 1 inch, and at the bottom 3⅜ inches, the total weight being taken at 1,772 tons, at a cost of £12 per ton, brings the cost of the Tubbing alone for the two pits to more than £40,000. The bottom of the hard rock was bored through at a depth of 931 feet, and below this, before reaching the impervious Coal-Measures (in which the Moss-Box will be laid at a depth of 1,030 feet) 80 feet of running-sand, gravel, and clay were bored through, and a wrought-iron tube was inserted to protect the sides until the main Tubbing is lowered down.

depth of the height of the teeth of the Trepan, thus leaving a solid ring of stone round the edge of the pit; the Trepan was again withdrawn, and after the outside teeth had been replaced, the boring was recommenced close on one side of the embedded tooth, and continued until the other end of the Trepan reached it on the other side, when it was lifted over and commenced work again on the other side. This was continued until the whole of the ring of stone was removed, excepting two small parts opposite each other, on one of which the embedded tooth lay. A few sharp blows of the Trepan released the embedded tooth, which was then without trouble picked up by the Grappling Tongs. When it is remembered that this operation was performed at the bottom of a pit 258 feet from the surface, and full of water, the skilfulness of the arrangements will be appreciated.

General Results.

It will be seen from Tables in the Appendix that the absolute time taken from commencing to finishing the Boring was one year five months in No. 1 pit, and one year seven months in No. 2 pit. There was, however, a delay of several months in No. 2 pit on account of the Tubbing not being ready; the depth of boring was also 40 feet greater. The time occupied in lowering the Tubbing and concreting, &c., was three and a half months in No. 1 pit, and four months in No. 2 pit. The total time taken to complete each pit was one year eight and a half months in No. 1 pit, and one year eleven months in No. 2 pit.

The average distance bored in No. 1 small pit in the Limestone was 1 foot 3¼ inches per shift of twelve hours, and in the Coal-Measures 1 foot 8¾ inches. In No. 1 large pit in the Limestone it was 7¾ inches, and 8¼ inches in the Coal-Measures. In the small No. 2 pit the average distance bored in the Limestone was 10½ inches per shift of twelve hours, and in the Coal-Measures 1 foot 4 inches. In No. 2 large pit in the Limestone it was 8½ inches, and 9¼ inches in the Coal-Measures.

The success of the Kind-Chaudron process at Marsden may be attributed—

1st. To the primary adoption of a size of pit no larger than had previously been successfully completed by this process elsewhere.

2nd. The use of tools which had already been thoroughly tested in the sinking of a previous pit.

3rd. In the purchasing of the necessary additional tools from firms accustomed to their special manufacture.

4th. To the excellent workmanship and quality of the metal of the Tubbing supplied by the Elswick Ordnance Works.

5th. To the entire absence of soft strata in the shafts.

6th. To the efficient and experienced staff of officials supplied by the Kind-Chaudron Company for the carrying out of this work; and to the cordial co-operation of the Belgian and English engineers, foremen, and workmen.

The terms of the contract were that no payment had to be made to the Kind-Chaudron Company for the patent right and superintendence unless the following conditions were fulfilled,—that the Tubbing when completed should not be more than 6 inches out of the perpendicular, and not let pass more than 40 gallons of water per minute. On the formal examination by the Engineers of the Whitburn and Kind-Chaudron Companies, it was found that in No. 1 pit the Tubbing was only 1 inch out of the perpendicular, and let pass about 1 gallon of water per minute, and this only at the wedging joint below the Moss-Box. In No. 2 pit the Tubbing was only 2 inches out of the perpendicular, and no water passed. In both cases the Tubbing itself from top to bottom was absolutely dry.

The Paper is accompanied by numerous diagrams and small scale drawings, from which Plates 1, 2, 3, and 4, have been prepared.

APPENDIX.

I.—KIND-CHAUDRON SYSTEM OF BORING.

Results of Boring in Nos. 1 and 2 Pits.

Particulars.	No. 1 Pit.		No. 2 Pit.	
	Small Trepan.	Large Trepan.	Small Trepan.	Large Trepan.
Commenced boring	Sept. 24, 1877	Dec. 4, 1877	Oct. 20 1879	May 4, 1880
Finished boring	July 17, 1878	Jan. 4, 1879	Apr. 26, 1880	May 21, 1881
Working days occupied in boring .	112	184½	162	327
Working days occupied in cleaning out	..	93	..	..
	Ft. In.	Ft. In.	Ft. In.	Ft. In.
1. Depth of boring in Limestone . .	181 5	181 6	222 9	222 9
2. „ „ „ „ Coal-Measures .	74 5	36 3	81 11	43 5
3. Total depth of boring	255 10	217 9	304 8	266 2
	No.	No.	No.	No.
4. Total number of working shifts (A)	187	322	309	363
	Ft. In.	Ft. In.	Ft. In.	Ft. In.
5. Average advance per shift in Limestone	1 3¼	0 7¾	0 10½	0 8½
6. Average advance per shift in Coal-Measures	1 8¾	0 8¼	1 4	0 9¼
7. Average advance per shift throughout	1 4½	0 7¾	0 11¾	0 8½
	No.	No.	No.	No.
8. No. of descents of Trepan in boring.	272	348	345	375
	Hrs. Min.	Hrs. Min.	Hrs. Min.	Hrs. Min.
9. Useful time actual boring . .(B)	1,573 30	2,325 15	2,702 45	2,390 0
10. Average proportion of shift occupied in boring (out of 12 hours) = $\frac{B}{A}$.	8 18	7 31	8 44	6 35
11. Time occupied in lowering, raising, and changing the Trepan . (C)	239 0	535 0	316 0	447 0
12. Average proportion of time in each shift = $\frac{C}{A}$	1 16	1 40	1 1	1 13
	No.	No.	No.	No.
13. Number of descents of the Sludger .	179	405	321	473
	Hrs. Min.	Hrs. Min.	Hrs. Min.	Hrs. Min.
14. Total time occupied in lowering, working, and changing the Sludger . . . . (D)	190 25	522 45	272 15	921 0
15. Average time of shift occupied in Sludging = $\frac{D}{A}$	1 1	1 37	0 52	2 32
16. Cleaning out small pit	..	2,046 0	..	377 0
17. Stoppages, ordinary	109 15	273 45	84 30	734 0
18. „ accidents	155 30	445 15	50 00	310 30
19. „ sundry	12 00	63 15	36 00	632 00
Total stoppages	226 45	782 15	170 30	1,676 30[1]

[1] This includes stoppages while waiting for the Tubbing.

II.—DIARY OF TUBBING AND CONCRETING IN NOS. 1 AND 2 PITS.

Date.		Particulars.	Time Occupied.	
No. 1 Pit.	No. 2 Pit.		No. 1 Pit.	No. 2 Pit.
1879	1881	Commenced boring	Sept. 24, 1877	Oct. 20, 1879
		Finished ,, ...	Jan. 4, 1879	May 21, 1881
Jan. 4	May 21	Removing boring tools, erecting bevil wheels, &c.	6 shifts	13 shifts
		Preparing Nos. 0, 1, and 2 cylinders and false bottom	11 ,,	17 ,,
		On inspection	..	8 ,,
,, 25	July 6	Commenced to lower the Tubbing.		
	,, 9	Tubbing floating	9	3
Feb. 19	,, 27	Lowering to bed and compressing the moss	13 } 22 ,,	12 } 15 ,,
		Filling the Tubbing with water by the siphon, and fixing a platform at the top of Tubbing, and preparing for concreting	3 ,,	3 ,,
Feb. 24	,, 30	Commenced concreting.		

Composition of Concrete.

No. 1 Pit.					No. 2 Pit.			
Height Filled.	Sand.	Lime.	Cement.	Gravel.	Height Filled.	Sand.	Lime.	Cement.
Met. Met.					Met. Met.			
0 to 35	4	2	½	..	1	Pure Cement.		
35 ,, 47	2	1	⅛	¼	1 to 6	2	1	½
					6 ,, 75	2	1	¼
82					82			

Date.		Particulars.	Time Occupied.	
No. 1 Pit.	No. 2 Pit.		No. 1 Pit.	No. 2 Pit.
Mar. 14	Aug. 22	Finished concreting.		
		Time lowering in concrete	19 ,,	21 ,,
		Concrete allowed to set before commencing to draw the water from the inside of the Tubbing, during this period the pulleys and winches were removed from the pit top, and a flat-rope drum fitted on the crab-engine	17 days	9 days
Apr. 1	Sept. 1	Commenced drawing out the water .	10 shifts	10 shifts
,, 6	,, 7	Finished ,, ,, ,, ,, .		
	,, 8 to 12	Removing false-bottom, pipes, &c., and drawing water.	5 ,,	7 ,,
	,, 12 ,, 22	Preparing the pit top and altering drum and pulley	10 ,,	10 ,,
,, 15	,, 22	Commenced sinking for false tubbing	16 ,,	22 ,,
,, 22	Oct. 3	Finished ,, ,, ,, ,,		
May 3	,, 4 to 11	Fixing Wedging-Cribs, Tubbing, and closing	20 ,,	15 ,,
		Total shifts	156 ,,	159 ,,

III.—General Results of Boring in Nos. 1 and 2 Pits.

Particulars.	No. 1 Pit.	No. 2 Pit.
Commenced boring	Sept. 24, 1877	Oct. 20, 1879
Finished „	Jan. 4, 1879	May 21, 1881
Days (including Sundays)	467	571
Distance bored by—Small Trepan	256 feet	304 feet
Large „	217 feet	266 feet
Average distance bored per shift of twelve hours, *i.e.* actual hours by—Small Trepan	8 hrs. 18 min.	8 hrs. 44 m.
Limestone	1 foot 8¼ in.	10½ inches.
Coal Measures	1 foot 8¾ in.	1 foot 4 in.
Large Trepan	7 hrs. 31 min.	6 hrs. 35 min.
Limestone	7¾ inches	8½ inches
Coal Measures	8¼ inches	9¼ inches
Lowering the Tubbing—Commenced	Jan. 25, 1879	July 6, 1881
Finished	Feb. 19, 1879	July 27, 1881
Concreting—Commenced	Feb. 24, 1879	July 30, 1881
Finished	March 14, 1879	Aug. 22, 1881
Foundation Tubbing—Commenced	April 15, 1879	Sept. 22, 1881
Finished	April 22, 1879	Oct. 3, 1881

IV.—Glossary of French Terms with English Equivalents.

French Term.	English Term.
Balancier	Lever working the rods.
Baraque	Covered Heapstead or Pit Hill.
Bétonage	Concreting.
Boîte à mousse	Moss-Box at bottom of Tubbing.
Cercle de raccon	Ring for carrying false bottom.
Cercle de suspension	Flange for lowering the Tubbing.
Chaine de suspension	Chain attached to rods at lever end.
Colon d'equilibre	Equilibrium column in centre of Tubbing.
Crochet de salut	Catching hook (Safety-tool).
Crochet double	Double hook.
Cuiller	Sludger.
Cuiller suspendue	Hanging sludger.
Cuiller de bétonage	Concrete sludger.
Cuvelage	Tubbing.
Cylindre batteur	Steam cylinder working the Lever.
Chûte libre	Freefall attachment above Trepan.
Clefs	Keys.
Dents	Teeth of Trepan.
Fanchère	Spear catcher (Safety-tool).
Faux cuvelage	Foundation Tubbing.
Faux fond	False bottom floating the Tubbing in descending.
Glissière	Sliding attachment above Trepan.
Grappin	Grappling hook (Safety-tool).
Machine cabestan	Crab engine working rope in pit.
Outils de sauvelage	Grappling or Safety tools.
Picotage	Wedging of Tubbing.
Piece elastique	Spring beam at end of Lever.
Tiges de sondage	Boring Rods.
Trepan grand	Large chisel.
Trepan petit	Small chisel.
Vis à allongement	Lengthening screw at top of rods.

V.—COST of BORING and TUBBING NOS. 1 and 2 PITS, MARSDEN COLLIERY, by the KIND-CHAUDRON PROCESS.

—	Construction.								
	Preparing and Erecting Baraque.[1]		Tools.[2]	Lining Tube.		Tubbing	Patent.[3]	Total.	
	Labour.	Materials.	Materials.	Labour.	Materials.	Materials.	Materials.	Labour.	Materials.
	£.	£.	£.	£.	£.	£.	£.	£.	£.
No. 1 Pit (12 feet diameter)	694	654	588	175	644	4,491	1,062	869	7,439
No. 2 Pit (13 feet diameter)	694	654	588	Nil	Nil	5,916	1,037	694	8,195

—	Working.								
	Boring.		Repairing Tools.		Tubbing.	Sundries. Salaries, &c.		Total.	
	Labour.	Materials Stores.	Labour.	Materials.	Labour.	Labour.	Materials.	Labour.	Materials.
	£.	£.	£.	£.	£.	£.	£.	£.	£.
No. 1 Pit (12 feet diameter)	2,033	577	825	282	457	3,708	722	7,023	1,581
No. 2 Pit (13 feet diameter)	1,765	278	660	261	125	1,723	216	4,273	755

—	Summary.		
	Labour.	Materials.	Grand Total.
	£.	£.	£.
No. 1 Pit (12 feet diameter) . .	7,892	9,020	16,912
No. 2 Pit (13 feet diameter) . .	5,967	8,950	14,917

[1] The Baraque was originally erected at No. 1 Pit; it was taken down and rebuilt at No. 2 Pit. The total cost is divided over both pits.

[2] The original cost of the Tools was £2,060; after the completion of the pits they were sold for £984. The difference is divided over both pits.

[3] Patent right (including plans and specifications).

Discussion.

Mr. Daglish. Mr. JOHN DAGLISH said, before the discussion upon the Paper commenced, he would describe the expansion-gearing attached to the large engine which actuated the pumps.[1] There were several difficulties in applying expansion-action to winding engines. The engine was reversed about every minute; it must be under such control of the engine-man that it must have full steam, at the commencement of the winding when full power was required, and at the termination to bring it to a state of rest; it should also be under such control as that at any moment full steam might be applied. The ordinary mode of working the Cornish valves of these large winding-engines was by a rocking-lever which moved the lever attached to the steam-valve, which rested upon it during the whole stroke. The first of these engines was erected by Mr. Barclay at Silksworth Colliery, in the year 1870. There was a piece of mechanism attached to the end of the lever. By lengthening or shortening the screw attached to this the lever itself was in effect lengthened or shortened, so that at a certain point the valve-lever slipped off the rocking-lever, and the valve fell. The appliance was variable, however, only through the action of the screw; it could be set to cut off at any point, but it was not automatic or varying strictly speaking; it had to be varied by hand to a set point. This appliance was greatly improved at the second engine that had been recently erected at the Silksworth Colliery by substituting a sliding-wedge for the screw. This wedge had the same effect as the screw. When the point of the wedge was just entered the lever was at its greatest length and the engine worked with full steam; when pushed through about half way the engine cut off at about half-stroke; when pushed to the full extremity the valve-lever was shortened so that the rocking-lever did not touch at all, and the valve was therefore not lifted. The sliding-wedge, by a mechanism that could be seen in the larger diagram, was attached to a governor. This was the first winding-engine in England to which a governor had been attached. The action was very satisfactory in steadying the engine. It would be observed from the indicator diagram that during the first four strokes of the engine the steam was at full pressure; the governor now came into action and gradually

---

[1] North of England Institute of Mining and Mechanical Engineers. Transactions, vol. xxv., Plate 52; and vol. xxix., Plate 1.

cut it off as the velocity increased from the sixth to the nineteenth stroke; then at the last stroke full steam was again put on. That description of expansion valve-gearing had been alluded to in a Paper by Messrs. T. Forster Brown and G. F. Adams, in reference to an engine that was erected at Harris' Navigation Colliery in 1878.[1] Mr. Daglish.

With reference to the Paper on the Marsden sinking, he had been asked to mention that the operation of sinking in the way described consisted of two distinct operations; first, boring the pit completely through to the required depth in the water; and secondly the lowering of the Tubbing in one operation, by attaching, piece by piece at the top of the water, until it reached the bottom, and the Moss-Box was compressed, and after that the filling in of the cement behind it.

Mr. Lupton. Mr. Arnold Lupton said that he had had the opportunity of seeing the sinking at Marsden whilst it was in operation, and he hoped he should not be considered presumptuous if he ventured to express his admiration of the excellent work that had been carried out. The question of sinking through water-bearing strata where the amount of water was too great to permit of sinking by the ordinary method, had engaged the attention of engineers on the continent for forty years, and he believed it was nearly thirty years since the first successful sinking and tubbing was accomplished by Messrs. Kind and Chaudron. With regard to the shaft-sinking by this method, he might mention that his attention had been first directed to the subject by the engineers with whom he was connected nearly eighteen years ago, who went to France and recommended the adoption of the process in the sinking of some pits in the Moselle coal-field, and he believed their recommendation was very successfully carried out. As to the special method described by the Author, it would be remembered that he had mentioned that the shaft first sunk had three diameters, 4 feet 11 inches, 5 feet 5 inches, and 14 feet, the reason had not been given; but he supposed it might be that the chisel falling in a straight line across the shaft struck each successive blow, as the tool was turned, more closely near the centre of the shaft than near the circumference. If the hole was bored all at one time there was some waste labour in striking the middle of the shaft, and it had therefore been found economical to bore out the centre of the shaft. But another engineer had sunk the shaft in one operation, which was described by Mr. A. Demmler, in a

---

[1] Minutes of Proceedings Inst. C.E., vol. lxiv., p. 23.

Mr. Lupton. Paper read before the Manchester Geological Society in 1878,[1] in which he had mentioned how Messrs. E. Lippmann and Co. had bored a shaft to the depth of 96 yards. Beginning at 12 yards from the surface where the water overpowered the pumping apparatus, they proceeded to sink by boring to a depth of 96 yards, where they put in the foundation of the Tubbing. The cutting was 14 feet in diameter, and the shaft when complete was 12 feet in diameter, the same size as that of No. 1 shaft described by the Author. In order to overcome the difficulty to which he had alluded of the straight cutter making the blows more closely together near the centre of the shaft, and not sufficiently closely together near the periphery, they had a different description of cutting tool, which was shown in Fig. 1, p. 200. The drill was in the shape of a double Y fastened together. There were two blades at that part of the tool which cut round the circumference of the shaft, and only one blade cutting in the part of the shaft which was at the centre, so that there were more blows in cutting the stone near the periphery than at the centre. Owing to the angle at which the blades were placed the stone was cut into checks or squares, so that it was broken up more certainly into small pieces. Not only was there that advantage but, owing to the breadth of the tool, when it struck the ground there was less liability for it to be deflected sideways if it happened to strike upon hard stone. He had read that in some cases a shaft sunk with simply a straight cutter, was sunk not quite perpendicularly, owing to that tendency to deflection; and Messrs. Lippmann claimed that their shape of cutter overcame this. Of course it was a great advantage if only one hole had to be bored, because it appeared that the shaft could be bored all at once as quickly as the enlargement could be done after boring the centre, as in the ordinary Kind-Chaudron method. The rate at which the work was done by Messrs. Lippmann, in sinking the pit in Westphalia, was about 10 inches every twenty-four hours, compared with 7 inches for every twenty-four hours' work in the shaft sunk by Mr. Daglish. In each case the rate was obtained by dividing the depth bored by the total number of days from the beginning to the conclusion of boring operations. Taking into consideration the time occupied, not merely in boring the hole but in tubbing it also, the speed was about in the same proportion. It therefore appeared that there was some advantage in using that shape of cutter; still, it should be borne in mind that no comparison of speed was of any value without taking into consideration the relative hardness of the strata. He had

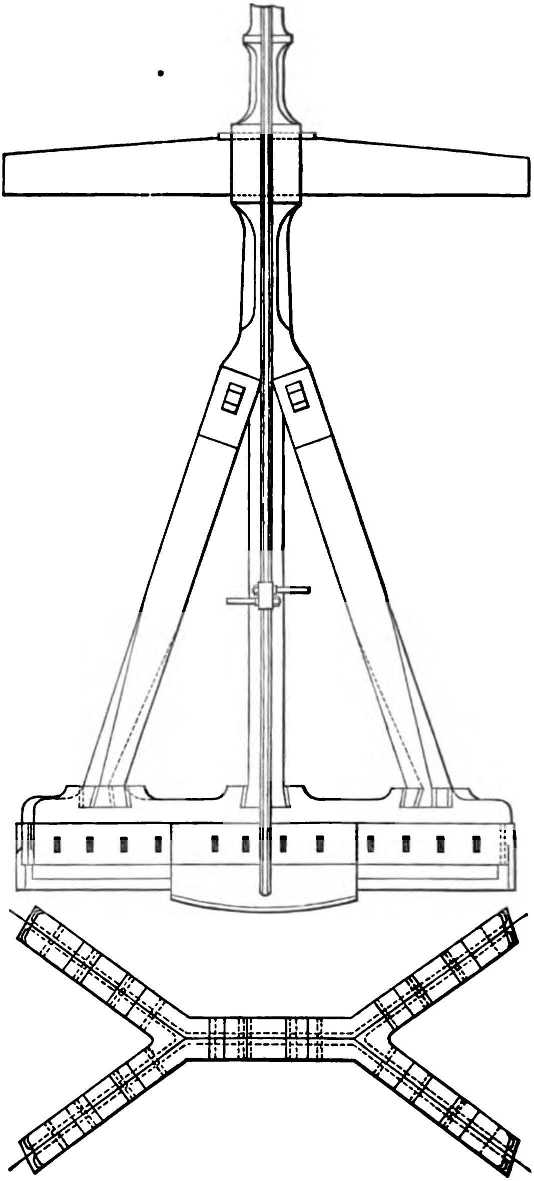
---

[1] Transactions, vol. xiv., p. 374.

not seen the pit in Westphalia, and therefore could not say **Mr. Lupton.** whether the stratum was as hard as that which the Author had bored. Very likely it was not. The cost seemed to be about the same, £170 per yard, when finished, including the cost of all the materials and the Tubbing. He might be permitted to refer to another method, in which he had been himself engaged, of overcoming a similar difficulty to that which the Author had encountered in Durham, the point of similarity being that a great body of water had to be encountered without pumping. The case to which he referred was in North Wales, where it was necessary to sink through a bed of very fine running quicksand on the seashore, where pumping was of no use, because not only was there a great body of water to deal with, but the sand ran in, and it was almost impossible to sink a pit by pumping. The compressed-air process was therefore adopted, which was commonly used in sinking the cylinders of bridge foundations. That process had been adopted in France for sinking a coal-pit upwards of forty years ago, but only on a very small scale. The instance to which he had referred in Wales was, he believed, the first of the kind in this country. The shaft was sunk down to a depth of 100 feet below the water-level by compressed air. There was a cast-iron cylinder 13 feet in diameter, in which air was pumped at a sufficient pressure to force out the water, the pressure of the air being exactly measured by the depth of the water. The tide coming in round the pit the level of the water was above the level of the land. By means of that process the sinking was easily performed, the workmen standing upon the sand and excavating it just as if they were on the surface and the tide was out. No doubt it was an operation that demanded some care in managing the men, who were subject to pains in their shoulders, which, in some cases that had come under his observation, made them dance and cry in a manner which was somewhat ludicrous to unsympathetic people, but if treated with care they did not suffer any great injury. It would take too long to describe the details of the process by which the Tubbing was connected with the solid ground at the bottom. He had, however, exhibited a diagram (Fig. 2, p. 201) showing the cylinder as it descended through the sand. Inside the outer cylinder was a smaller cylinder 6 feet in diameter, connected with a larger one at the bottom by a conical piece or bell. The pressure was inside the smaller cylinder, and in the conical piece at the bottom. At the top of the smaller cylinder was an air-lock, through which the men and materials could enter and leave. The pit was sunk through sand, boulder

Mr. Lupton.

FIG. 1.

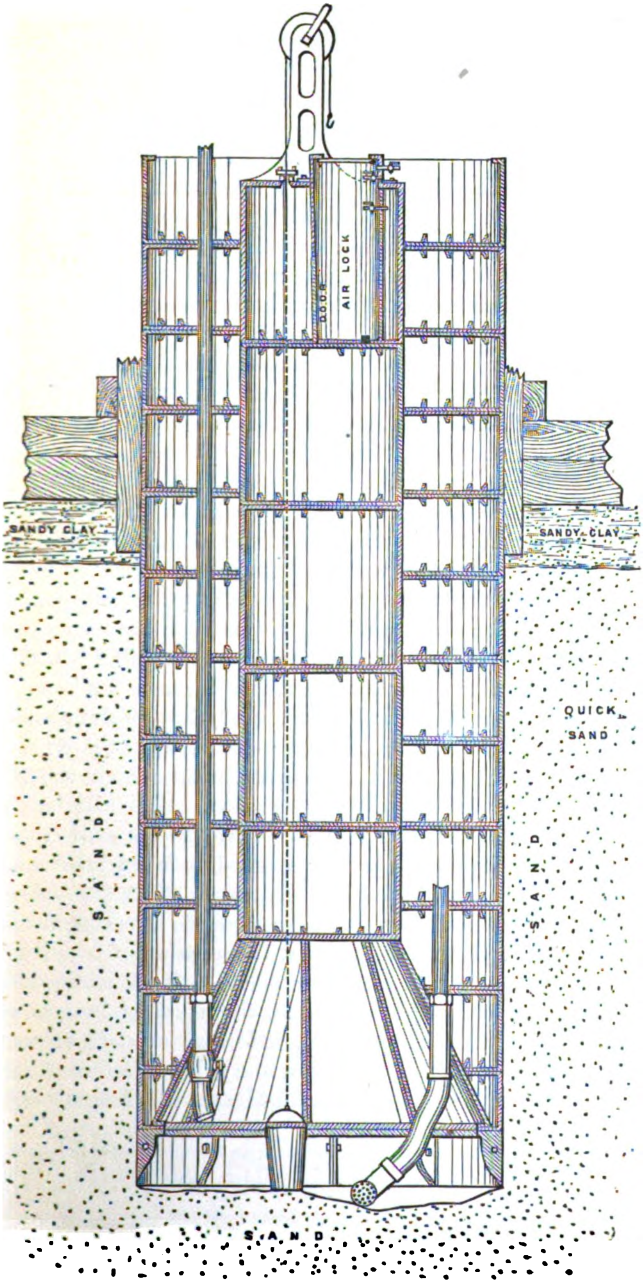


Scale $\frac{1}{30}$.

LIPPMANN'S BORING TOOL.

Mr. Lupton.

Fig. 2.—Compressed-Air Sinking at Bettisfield.



Scale 7 feet = 1 inch.

Mr. Lupton. clay, and gravel, into the red marl, where a firm joint was made with a solid stratum. The cost in that case was also about the same as that of the Kind-Chaudron process, about £170 per yard, including the cost of the materials left in the ground, and a fair proportion of the cost of materials used during the process. It had sometimes occurred to him that possibly the compressed-air process might be of advantage in making tunnels beneath estuaries or rivers, where the depth was not too great to admit a man working under pressure. He had often been under a pressure of 45 lbs. to the square inch above the atmosphere, or 4 atmospheres. Divers of course were sometimes under still greater pressure, but a pressure of 4 atmospheres was about as high as was comfortable for divers. No difference, however, was felt in the compressed-air cylinder between that pressure and the ordinary pressure of the atmosphere, except that one was apt to get rather warm with very little exertion.

Mr. Redman. Mr. J. B. REDMAN said the first application of the "plenum" process referred to by Mr. Lupton was the sinking of Tubbing by Mr. Triger, a French Engineer, through a semi-fluid stratum of sand to a coal-field on the banks of the Loire, near Chalonnes,[1] the particulars of which were given in the early editions of Dr. Ure's "Dictionary of Arts, Manufactures, and Mines," in the article "Ventilation of Mines." This was about 1840.

Mr. Smyth. Mr. WARINGTON W. SMYTH said he would confine his remarks to the subject of sinking by means of boring; and, secondly, to securing it against water by means of the Tubbing specially devised by Mr. Chaudron; but he desired first to express his approval of the clear and simple manner in which the Author had described the details of his operation. His criticism would merely be with reference to one or two points which, in the unavoidable brevity of the Paper, seemed to have been omitted. Mr. Daglish appeared to be scarcely aware that one important method of saving time, to which he had alluded, had been adopted long ago by Mr. Chaudron, namely, the placing of a cylinder or conical receiver suspended in the smaller shaft to receive the broken material coming away from the sloping bottom of the larger shaft during the operation. He had seen it at work in 1871, and he then read a Paper on the subject before the North of England Institute of Mining and Mechanical Engineers.[2] He

---

[1] Comptes Rendus hebdomadaires des séances de l'Académie des Sciences. Tome Treizième, 1841, p. 884. *Also* Annales des Mines. Quatrième série. Tome ix., p. 349.

[2] Transactions, vol. xx., p. 187.

found the plan was already in operation at the pits of Maurage. Mr. Smyth. Moreover, members ought to be reminded that the same arrangement was to a certain extent adopted many years ago by a famous French borer, Mr. Mulot, who he believed was the first person to attempt to replace the ordinary method of sinking by boring on a large scale. He had carried out certain operations of that kind in the Pas de Calais, where he had made borings on a considerable scale as far back as 1839; and in 1849 he bored one hole of 13-feet diameter for the purpose of a shaft, putting in a smaller shaft which he sank at the commencement, and a suspended cylinder or recipient to which at the time many engineers had taken exception. Then came the very remarkable bore-master, Mr. Kind, who was originally a simple Saxon mining captain, but a man of the highest intelligence and honesty in all that he carried out, who proposed systematically to sink shafts by boring. It was the ingenious classes of apparatus devised by Mr. Kind that had enabled the shafts to be bored with such facility, certainty, and immunity from accident. He was the first person to employ, and was recommended to take a patent for, the introduction of wooden rods instead of iron rods, which by their greater power of floating in water undoubtedly enabled the Germans to make a series of bore-holes exceeding in depth those in any other part of the world, and also in the economy with which they had been carried out. Mr. Kind proceeded to a very great extent with remarkable success. When the district of the Moselle was being explored for coal, he put down shafts successfully by means of borings, the shafts standing full of water, without introducing any pumping arrangements, and he succeeded in a boring at Steyringen in the Moselle, about the year 1848. He would have succeeded entirely but for the grand difficulty, which was afterwards solved by Mr. Chaudron, namely, that after his bore-holes had been carried down of sufficient size for a coal-shaft through all the difficult ground which they had to intersect, into the Coal-Measures below, he failed to establish a secure base for the Tubbing. The attempts to pump out the water were unsuccessful, and for several years the whole process remained to a certain extent in abeyance. Mr. Chaudron then came to his assistance, and, with the simple and beautiful arrangement of his Slide-ring and the Moss-box at the back, was enabled to put down a series of shafts with unvarying success. In Belgium, in France, in the district of the Moselle, and in Westphalia, he had sunk, at the time of Mr. Smyth's introduction to him, no fewer than twelve or fourteen shafts, each beset with difficulties, but each

Mr. Smyth. entirely successful in its results. No doubt the union of those two men had been the means of introducing one of the most remarkable improvements ever effected; whatever the quantity of water, they were enabled to cope with it without the introduction of the expensive arrangements needed for pumping. Scarcely an accident occurred, and in the end the water was substantially and thoroughly tubbed out from the shaft, so that further proceedings could be at once undertaken under the usual system of sinking. When he had the pleasure of examining the arrangements carried out by Mr. Chaudron in Belgium, he was sinking the two shafts at Maurage, in which a length of Tubbing weighing 800 tons had to be lowered by the simple but effective contrivance which he had introduced, and the operation was performed with such success that the water was entirely plugged out, and further workings could be carried on in safety and without difficulty. Those examples, crowned by that of the Whitburn sinking, had taught engineers two important lessons in respect to the two divisions into which the subject naturally fell. In his whole series of borings, including the remarkable well at Passy, and a number of other Artesian wells, besides his wider shafts Mr. Kind might be regarded as unrivalled in fertility with regard to the general arrangements for boring and the contrivances he had introduced on all sides for easily and securely carrying out the lowering and lifting, the arrangement of the rods and the like, by a very moderate-sized steam-engine, and sparing as far as possible the large number of men who were generally considered necessary in a great sinking undertaking. But it should be remembered that unless the process was carried out with the simplicity which belonged to the true Kind-Chaudron process, and which the Whitburn Company had, through Mr. Daglish, adopted in its entirety, very serious dangers and difficulties might be encountered. He made that observation especially because he had been conversant, through the introduction of Mr. Chaudron, with the sinking of several other pits in Belgium, where, after an examination of his process as carried out in the Moselle, an effort had been made to do the work without touching his patent, by means of some little modification which had ultimately resulted in total loss or destruction of the undertaking. One example had struck him forcibly at Havré, between Manage and Mons, where the district was of the most difficult character to deal with, having a quantity of extremely water-logged layers or strata lying between the surface and the Coal-Measures, near where Mr. Chaudron was at the time successfully carrying out his system. He had kindly given Mr.

Smyth a letter to a most ingenious engineer, Mr. Bourg, who, at a colliery placed on the line south of and close to the railway, had, after seeing Mr. Chaudron's method, nevertheless thought fit to adopt a method of his own. He commenced by putting down three shafts within 50 metres of each other, the one to help the others, then erecting three powerful direct-acting steam-engines, one over each of the shafts, and each attended by a winding-engine. He went to work pumping the water in the old style, passing through the same series of difficult measures that Mr. Chaudron had to deal with in the shaft nearly alongside. He saw that shaft nearly completed. Mr. Bourg, who generally kept his doors closed against visitors, kindly permitted him to see his arrangement, which was certainly very ingeniously carried out. His plan of pumping the water out by three contiguous engines entirely failed. Mr. Bourg then, after a partial use of compressed air, adopted a system closely approaching Mr. Chaudron's, and employed cutters as nearly as possible of the same form, lowering the Tubbing as he proceeded, armed with a sharp edge at the bottom, which was to cut into the lower measures as soon as he had got through the watery ground. He need not enter into the details, but it was interesting to compare them with the much simpler arrangement of Messrs. Chaudron and Kind. The method adopted was that of a treble cutting tool, the upper part of which was composed of a cutter like the large Trepan shown in Plate 3, Fig. 3, of 14 feet or 15 feet diameter; below it a smaller cutter, to cut out a preparatory bore, and below that a recipient tool to receive the dirt and gravel as they were cut away, and bring them to the surface. The operation was a complicated one. It went on for a short time, but the breakages were frequent, and in the course of a few weeks the system was found to be a total failure. When they attempted the lowering of the Tubbing equal difficulty was experienced, and at the time he was there they had only done about a third of the work. They had to put in a smaller inner Tubbing, but again found so many difficulties that, after working from the year 1864 to 1871, they had not approached the principal difficulty of the whole affair, namely, the shutting off the water from the interior of the shaft. It was estimated by engineers in the neighbourhood that the works had cost about £140,000, and there was not the least appearance of their coming to a conclusion. He had not heard whether the winning had been completed, but he very much doubted it. He merely mentioned those facts in order to show that the simplicity of the arrangement that had been described in the Paper constituted one of

Mr. Smyth.

Mr. Smyth. the great virtues of the system, and that the Company had bee well advised in adopting the method pure and simple, instead c endeavouring to import into it any novelties which, like those c Mr. Bourg, might appear to be extremely judicious and ingenious. Having known the great Whitburn undertaking from th time when it utterly failed as a simple sinking, from the tim when descending the shaft he saw two 30-inch pumps working at extreme speed, besides several smaller ones, amidst a sea of rushing water, tumbling to and fro—having known it in that state, and having within the last two years seen all those difficulties surmounted, he thought there was every prospect that millions of tons of good coal might be drawn up through those shafts, and that engineers might take heart from such an example in the expectation of being able to go down in the midst of salt-water, and safely reaching the mineral treasures, which they would find in the watertight Coal-Measures below.

Mr. Galloway. Mr. W. Galloway observed that as early as 1823 bore-holes of from 12 to 18 inches in diameter were sunk in order to facilitate the ventilation of the workings. In 1843 Mr. Kindermann took out a patent for sinking shafts and lining them with a watertight lining, and during the last years of his life, from 1843 to 1848, he sank no fewer than seventeen such shafts at depths varying from 4 to 29 fathoms. His process of making a watertight joint at the bottom of the cylinder was somewhat similar to that of the Moss-box; but instead of using moss, like Mr. Chaudron, he took a linen cloth rolled up and steeped in some kind of adhesive substance, and this was compressed against the ground by the bottom of the cylinder as it passed over a ring in the bottom of the shaft, exactly in the same way as in Mr. Chaudron's process. As Mr. Warington Smyth had referred to the commencement of Mr. Kind's career, he need not further allude to that subject; but, in regard to possible improvements in the system, he might be permitted to mention that Mr. Chavatte had a colliery in the north of France, and had made a sinking through 34 metres of wet running ground, and 103 metres of chalk down to the top of the Coal-Measures. The whole of the sinking was done under water by Mr. Chavatte himself, without any assistance from Mr. Chaudron; his reason for attempting it being to avoid the payment of a premium of 75,000 or 80,000 francs. In sinking through the running ground he commenced with a ring of masonry about 2 metres in depth and 15 feet interior diameter. Inside of that, he placed a sheet-iron cylinder and then commenced with what the Germans called a "sackbohrer," working the shaft down in

Mr. Galloway.

that way, and pressing the cylinder down by means of screws. He had lowered four cylinders of that kind, one inside the other telescope fashion, before he reached the bottom of the running measures, 34 metres in depth. At last, having reached that depth, he had not to line the shaft in any way with a temporary lining, the ground being good, but he proceeded to bore it out. He then bored out the remaining 103 metres with ordinary tools, such as those of Kind and Chaudron, with a diameter of 4·24 metres, and he introduced the same kind of cast-iron Tubbing. Instead of using the Moss-box, he placed a ring in the form of a truncated cone at the bottom of the cast-iron cylinder, and floated it down in the same way as was done by Mr. Chaudron. He had contracted the boring somewhat at the bottom, so that when the truncated cone came to rest on the bottom, it crushed away a little ledge that had been left, and made what was thought to be a watertight joint. No dependence, however, was put upon that joint, but concrete was inserted in the most careful manner. Mr. Chavatte thought that he had introduced a better method of concreting than that used by Mr. Chaudron. He did not use four windlasses, but only two, and on each windlass there were two ropes, one of which let down a box of concrete while the other drew up the empty box, so that instead of men being employed alternately pulling up an empty box and letting down a full one, the full box helped to draw up the empty one. Mr. Chavatte thought that in the Chaudron process sufficient care was not used in making the concrete tight enough, and that if that were done there would be fewer failures. He alleged that in the Chaudron process eight out of every ten shafts leaked to a large extent after the Moss-box had been compressed, but whether that was the case or not, Mr. Galloway was not prepared to say. He might mention that Mr. Chavatte's article was to be found in the "Bulletin de la Société de l'Industrie Minérale."[1]

Mr. Fox.

Mr. FRANCIS FOX (of Sir Charles Fox and Sons) said that, having on more than one occasion visited Whitburn during the progress of the sinking, there were one or two points to which he should like to direct attention. The great difference noticeable between that method of sinking a pit and the ordinary system, was the exceedingly small number of men seen about the works. Hardly anybody was visible. There was the Belgian foreman standing with his hands behind his back and a pipe in his mouth, uttering a word or two here and there to the others, and every man

---

[1] Deuxième Série, Tome xi., p. 767.

Mr. Fox. seemed to know exactly where to put his hand upon the tool necessary at the moment. The system of making the Tubbing float until it came to a final rest at the bottom of the shaft was admirable, but it was a matter requiring great care, as was proved at Cannock Chase. There, owing, as he was informed, to the carelessness of some workmen, the siphon pipe, used for letting in water from the outside of the Tubbing to the inside, to bring the Tubbing to its proper line of flotation, was allowed to run too long. The result was that the whole of the Tubbing went to the bottom. That, however, was not due in any measure to the system, but simply to a breach of the rules, and the failure could in no respect be attributed to the Kind-Chaudron method. The Tubbing under discussion was "open-top" Tubbing; in other words, there was no wedging crib or "up-over" crib as in some pits. In a shaft where it was necessary to place an "up-over" crib so as to tub off water in a certain stratum, theoretically the pressure behind the Tubbing was simply due to the head. It had been found in many cases that the Tubbing had cracked and blown, and it was difficult to account for it. It was made thicker and thicker, but no matter how thick, the inevitable result was that it became cracked, or some catastrophe happened. He believed it was Mr. William Coulson of Durham who had suggested the idea of putting a small safety-pipe up the shaft to allow any gas or air that might accumulate in the stratum behind the Tubbing to escape. He could not explain, and he thought it had never been explained, how it was that the air or gas behind the Tubbing could possibly have a greater pressure than was due to the hydrostatic head; yet it was so, and ever since the insertion of that small pipe, about 2 inches in diameter, and carrying it through the "up-over" crib, bringing it sufficiently high in the shaft above the water-level, no failure had occurred. It was stated in the Paper that the 30-inch pumps were worked at seventeen strokes per minute. That was on the 6-feet stroke, which gave a bucket-speed of 204 feet per minute. Those identical pumps were now in use at Liverpool and Birkenhead in connection with the Mersey Tunnel, of which Mr. Brunlees, Vice-President, was one of the engineers. The pumps were worked regularly at about 7½ strokes on a 10-feet stroke, which gave 150 feet bucket-speed. He thought it unadvisable to let them run at a higher speed, but, as admitted in the Paper, it was a case of extreme pumping, and the method was adopted as a desperate resource. It was no doubt due to the admirable design of the pumps, and the machinery connected with them, that no breakage

Mr. Fox.

had occurred. At Birkenhead 20-inch pumps had been run with a direct-acting engine, at seventeen strokes per minute, but he had always thought it unadvisable to adopt so high a speed. The pneumatic system of sinking would of course apply to comparatively shallow depths, and not to any depth over 100 feet. He believed the bridge at St. Louis was an instance of the greatest depth in which the pneumatic system had been applied, namely, 113 feet. The results in regard to the workmen were more objectionable than those which had been described, because at one of the piers twelve men were lost from what the Americans called the "caisson disease." At Omaha bridge all the piers were sunk by the pneumatic system, but it was a recognised rule that that system should not be adopted until every other device had been tried. It was found at Omaha that the ill effects produced upon the men working under pneumatic pressure were due to the varying pressure of the air, even if only to a very slight extent. It was therefore essential, in all pneumatic work of that kind, that there should be a reservoir of air compressed to a high degree, and arranged with an equilibrium valve, so as to keep the pressure of the air exactly the same. During last spring he visited a tunnel under the Hudson river, which was being driven by compressed air, but which would have been driven by the Kind-Chaudron or any other system, if only it could have been applied in a horizontal direction; this was, however, impossible.

Sir G. Elliot.

Sir George Elliot, Bart., M.P., being connected with the county in which the sinking under discussion had been made, was induced to say a few words upon the subject. He remembered the sinking of a number of deep collieries, such as Monkwearmouth, Seaham, Seaton, Merton, Haswell, South Hetton, Shotton, and Harton, some of which had cost as much as £250,000 or £300,000 to sink, and he believed the sinking of the Whitburn Colliery itself, before the new method was adopted, had cost upwards of £100,000. He had taken the opportunity of going over it with Mr. T. E. Harrison, Past-President, two years ago, when the work was in progress, and he was astonished to see the certain manner in which it was conducted. He thought that in no part was such a demonstration so necessary as in the county of Durham. The coal-field there rose to the west, and proceeded to dip to the east, taking the coast line for 18 or 20 miles, and passing under the sea at a great depth. The inference to be drawn was that, as the coal extended from the sea westward to a distance of 25 or 30 miles, there was no good reason why it might not extend a long way under the sea. The experiment was a bold

Sir G. Elliot. one, and nobody in the north of England anticipated success when it was begun. He had seen nearly all the sinkings except Hetton Colliery, which was a failure to begin with, and he had great doubts as to the possibility of sinking in that way so simply and inexpensively as compared with the mode in general use for the previous fifty years. Now that the result had been accomplished, he thought engineers had within their reach the means of exploring the coal-fields by very deep sinkings. Those in Durham were generally in Magnesian Limestone, containing a large quantity of water, and often a large thickness of quicksand at the bottom of the limestone, but by the new method the coal might be reached with some degree of certainty as to cost. Until the present experiment had been made it was impossible for any mining engineer to form an estimate, first, as to whether the thing could be done, and secondly, as to what the cost would be; but the experiment at Whitburn had reduced the operation to a certainty. He knew well the coal-fields in Durham and Northumberland, and he was a member of the Royal Commission appointed to ascertain what amount of coal there was in the country. He believed that, taking the output of the coal-fields in Durham and Northumberland as at present, with no further increase, there was not coal enough down to the coast to last one hundred years. It was perhaps a bold thing to say, but that was the result of calculation, and he thought it was correct. By the method in question, however, the time might be considerably extended, as it would be possible to penetrate further, and, if necessary, to go into the sea. He saw no difficulty in making sinkings there. Of course it would be expensive, but small islands might be constructed and pits sunk, so that the coal might be worked to any distance from the coast. The new method made it certain that coal could be reached in the remotest parts under the sea, and thereby the great source of wealth lying under the ocean in that part of the country be realised.

Mr. Thomas. Mr. J. L. THOMAS believed that the Kind-Chaudron process of shaft-sinking was destined to exercise a marked and beneficial influence on mining where minerals had to be reached through water-bearing strata. The Author did not say what diameter it was originally intended to sink Nos. 1 and 2 shafts, and to what extent the size of the shafts had been altered; but he mentioned that a diameter of 14 feet 3 inches was chosen for the shafts to be sunk by the Kind-Chaudron process at Marsden, because that was the largest size hitherto sunk by this system on the Continent. Mr. Thomas believed that a shaft on this system had been commenced in the Cannock coal-field, the diameter of the boring of which was 19 feet, and the diameter inside the tubbing 15 feet. Should circumstances require it he believed that the size of the shafts described in the Paper might be safely exceeded. Mr. Thomas.

The first operation appeared to have been the lowering of a wrought-iron tube 54 feet long and 14 feet 4 inches in internal diameter. He would ask whether this tube was lowered in one piece or in sections? In boring No. 1 shaft a small Trepan 4 feet 11 inches in diameter was adopted, its weight being 7 tons, the boring-rods being formed of pitch pine timber 5 inches square. He would enquire whether the same size of boring-rod was used with the large Trepan as with the small one? also whether any breakage of the rods took place, and if so at the iron terminations; and further, whether the Author's experience had led him to the conclusion that steel could be advantageously used for these terminations? Pitch pine was often employed for the main pumping-rods in Cornish pitwork, and could be procured in suitable lengths. When clean and free from knots it was a good material to use where toughness under jars and unequal strains was essential.

The Author stated that in very hard rock comparatively few and light blows were given by the boring apparatus; it would seem desirable, however, that the blows should be vigorous, and as many as possible, in dealing with such material. He would enquire what was the extent of fall of the Trepans, and the number of blows per minute given in boring through hard Magnesian Limestone with the large and small Trepans respectively? The hindrance to hard blows would, he presumed, be the jar on the rods; but that would be in some measure prevented by the sliding joint, or, in the event of that not being sufficient, by the Freefall.

The rate of progress of sinking seemed to have been greater for the small Trepan in shaft No. 1 than in No. 2. This he thought arose from the size of the Trepan in No. 1 shaft being 4 feet 11 inches and in No. 2 shaft 6 feet 6¾ inches. With the large Trepan, however, shaft No. 2 was deepened more quickly than No. 1; and this was no doubt owing to the greater size of the preliminary bore-hole, namely 6 feet 6¾ inches as against 4 feet 11 inches in No. 1 shaft. In the latter the bore-hole of the small Trepan seemed to have been kept from 30 to 40 feet in advance of the large boring; but this practice was apparently departed from in No. 2 shaft, in which the boring with the Trepan, 6 feet 6¾ inches in diameter, was at once completed to a depth of 383 feet, and at that depth a ledge formed for the Hanging Sludger to rest upon. He thought it was chiefly owing to this additional depth, from which the débris

Mr. Thomas. had to be removed, that in No. 2 shaft nine hundred and twenty-one hours were occupied in lowering, working and changing the Sludger, as compared with five hundred and twenty-two hours for the same work in No. 1 shaft. It seemed that at Marsden only two sizes of Trepans were used in each shaft; but he believed that it was not unusual to employ three; and he could understand even a greater number being employed under circumstances where the size of the boring and the hardness of the rock might render their use advantageous. Finally he would enquire whether the chisels or teeth were of steel or of iron faced with steel, and whether they often needed removing from the Trepan for repairs? One of the teeth appeared to have got away from the small Trepan, and to have been imbedded in the rock at the bottom of the bore-hole; did this occur from fracture, or from its having worked loose?

He feared the process would not be of much value in metalliferous mining, which was a tentative process necessitating the examination of the vein at frequent intervals during the sinking of the shaft.

Sir Frederick Bramwell. Sir FREDERICK BRAMWELL, Vice-President, said the Paper had not entered into the detail of the various tools shown in the drawings, and therefore he could not be sure that that which he was about to mention was amongst the tools used; but he did not gather that it was, and he could not recognise it among the drawings. Two or three years ago he was interested in a boring of small diameter, only 13 inches, but it was 700 feet deep, through Chalk and Gault, into the Lower Greensand. On reaching the Lower Greensand it came up into the bore-hole, and the question was how to clear it out. The implement which he had made for the purpose consisted of a cylinder 15 feet long, and of a diameter that would admit of its easily going down into the bore-hole. In the cylinder there was a piston, and at the bottom of the cylinder there was a foot-valve of the plainest character. The apparatus was lowered by a crab having two flat wire ropes, one attached to the piston and the other to the cylinder. The ropes were made flat so that there might not be any tendency for them to twist round one another as they hung in the bore-hole. The steam-crab was made specially, so that either rope-wheel could be unclutched, and could be held by its brake. During the lowering the ropes were paid out equally, so that the piston remained in the same place in the cylinder, namely, close to the bottom. As soon as the surface of the material in the bore-hole was reached, the rope which had lowered the cylinder was eased off by the wheel being unclutched while the piston-rope was hauled up. The result was to cause the cylinder to sink down in the manner of pneumatic pile-driving into the material that had to be raised, and to do so with very great rapidity, and at the same time to cause the material at the bottom of the cylinder to flow into it, urged by a pressure due to the head of 400-feet of water above the cylinder. Although the material, as a rule, was fine sand, there were occasionally found pieces as big as one's hand, and they were brought with the greatest ease through the bottom valve into the cylinder, and got out. The implement was extremely successful in getting up anything presented to it, as obviously under the heavy pressure the moving of the piston up the tube involved anything that was below it being driven in with great violence, and he did not know any limit of what would go in, except the size of the aperture of the tool itself. He should like to ask the Author, with regard to the valve-gear, whether there was any, and if so, what provision to check the fall of the steam-valve when the rocking lever, as in Figs. 3 and 4,[1] had quitted the arm, and had left the valve at liberty to fall. He gathered that the indicator diagrams 1, 2, 3, 4, represented the pressure required to get the heavy mass of the drum-wheel, and so on, into motion, and when the velocity was reached, that the governor came into operation; from stroke 6 to 19 there was a gradual increase in expansion. Sir F. Bramwell would be glad if the Author would explain why it was that, in the act of stopping the machinery, the full pressure was put on. He should like to know whether, in the act of stopping, the steam was not put on in the reverse direction, and whether there were not diagrams of that character? He had seen such diagrams, and they had given indications of what he had before mentioned; the ram action of the steam when going by back-gear into the cylinder, which he obtained in a very marked degree in some experiments on the Le Chatelier brake on the South-Western Railway twenty years ago. They showed this when the winding-engine was brought to rest by the steam being admitted when the engine was in reverse-gear.[2] Sir Frederick Bramwell.

Sir James Douglass. Sir JAMES DOUGLASS remarked that Mr. Daglish was a neighbour of his, having commenced operations close to one of the lighthouses of which he had charge. He well remembered the commencement of the works at the colliery, and the feeling that came over that part of the country when it was supposed that they were a failure. He had had the opportunity of watching the works

---

[1] North of England Institute of Mining and Mechanical Engineers, Transactions, vol. xxix., plate 1.

[2] Minutes of Proceedings Inst. C.E., vol. xxxvi., p. 119; vol. xxxvii., p. 24.

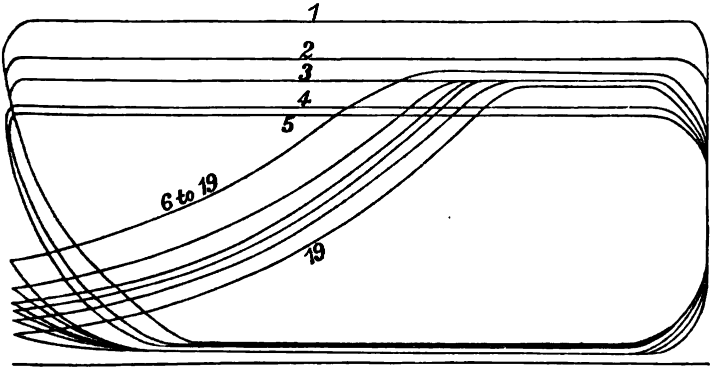
Sir James Douglass.

during their progress, and he desired to add his testimony as to the extraordinary skill and steady perseverance with which they had been conducted, their perfect machine-like regularity, and the absence of the tumult generally expected in large operations of that kind.

Mr. Daglish.

Mr. DAGLISH, in reply, said, that many of the points raised during the discussion had already been answered by other speakers. The question of the use of compressed air had been taken into consideration, but as the depth was known to be 300 feet, it was considered impossible to adopt that method. The number of men employed in the operation at one time was never more than five or six. The Cannock Chase pit was commenced after the arrangements had been made for the Marsden pit, or about the same time; 14 feet 3 inches was the largest size adopted on the Continent up to that time, and that was the reason why it was adopted at Marsden. The tube mentioned by Mr. Lee Thomas was one large piece, like a very large boiler, and it was lowered down within the part of the pit that had been excavated, by the ordinary operation of sinking, and then backed up with cement, so as to form a secure lining, and prevent stones falling during the rest of the operation. The rods were all of the same size, with iron terminations, the breakages of which were not unfrequent; but the various appliances for extracting the rods made it a matter of very little moment, because the extraction of the rods was a matter of certainty. Perhaps the iron terminations broke more than the rods themselves, but there were breakages of both parts. Respecting the rate of speed, it was found that when the rock was very hard there was great vibration of the rods, and time should be allowed for them to steady, before the blow could be repeated, so that the progress was slower in the harder than in the softer rock. As to the Freefall, he might state that in all the pits that had been sunk by this method there were more or less variations in the character of the strata; and the case at Marsden was special, because the character of the rock was entirely different, and he had been given to understand very much harder than it had been met with elsewhere. Of course the question of expense was very carefully considered throughout, and no tools were prepared or purchased except those that were necessary; therefore the Freefall, which was an expensive tool, was not prepared until it was found that the second pit was being sunk rather slowly. It could not be adapted to the larger Trepan. There was some little difference of opinion among the various engineers about it, but it was thought that the larger Trepan was too heavy for the Freefall apparatus. There was no such tool as that which Sir Frederick Bramwell had described in operation at Marsden, because there was no soft sand to extract. Very frequently pieces of the size of 2 feet were struck off the edge of the top of the small inner pit; indeed so large were they that they indicated the inclination of the beds, and in going through the Coal Measures that was a matter of great importance, because it was known exactly how the Measures were lying by the pieces brought by the Sludger. Tool Fig. 15, Plate 3, was an apparatus somewhat resembling that which Sir Frederick Bramwell had mentioned, but it was used for lowering in cement. It had a piston inside, but there was a piece of cord attached to it. It would be seen by the diagram that it was a loose piston, and when the tool was lowered rapidly behind the tubbing filled with cement—when it got to the bottom the rope was pulled and the contents were discharged by the piston. Sir Frederick Bramwell's observation was perfectly correct, that in ordinary coal winding the steam was thrown against the engine at the termination of the winding, especially so where the appliances which were attached to this engine, for counterbalancing or equalising the load, were not in operation. This engine had what was called a Scroll or compensating drum, by which the load upon it was equalised. In the case of engines that were not counterbalanced in some way, as in the great majority of winding engines in England, the steam was thrown against the piston at about half winding. Mr. Daglish.

FIG. 3.



The diagram (Fig. 3) was taken specially, and the engine was not running hard; therefore the compression line at the termination was slight. But it was an ordinary indicated diagram, Mr. Daglish. taken off the engine when going rather slowly, and exhibited slight defects in some respects. There was a certain amount of wire-drawing in the steam pipe. At the first stroke the engine was going very slowly, and it had full boiler pressure, but as it increased in velocity there seemed to be wire-drawing in the steam pipes or ports, and the pressure was reduced several lbs. until the cut-off came into operation—then the pressures increased again. There was a favourable action shown in the back pressure at the bottom. When the expansion was in operation there was slightly less back pressure than when the full steam was on throughout the stroke.

Sir J. W. Bazalgette. Sir J. W. Bazalgette, C.B., Vice-President, said that the subject of the Paper, and of the interesting discussion that had followed it, was one of the greatest importance to the Institution. It might be said to be a Paper on the means of obtaining a more ready access to those necessaries of life—water and coal. It was also a subject of peculiar interest to engineers who had to do with underground works, for they all knew that in water-bearing strata, when once they could subdue the water they had overcome nearly all the difficulties with which they had to contend. It had further been noticed that in passing through running sand the difficulties of pumping water without the sand had been very much diminished, and in pumping and excavating near buildings that was a matter of great importance. In his early days, making excavations in the metropolis was a work of great danger, and many buildings were allowed to settle from pumping in their neighbourhood. The means adopted for pumping water, and preventing the sand from being drawn out also, were now so improved that there was seldom anything of that kind occurring.

---

12 December, 1882.

Sir J. W. BAZALGETTE, C.B., Vice-President,
in the Chair.

The discussion upon the Paper "On the Sinking of Two Shafts at Marsden, for the Whitburn Coal Company," by Mr. Daglish, occupied the evening.

ANNUAL GENERAL MEETING.

19 December, 1882.

Sir W. G. ARMSTRONG, C.B., F.R.S., President,
in the Chair.

The notice convening the meeting having been read,

Messrs. A. T. Atchison, T. H. Blakesley, G. Chatterton, W. Dawson, J. M. Dobson, C. Frewer, D. Gravell, H. G. Harris, and T. M. Smith, were requested to act as Scrutineers of the Ballot for the election of the President, Vice-Presidents, and other Members of Council for the ensuing year; and it was resolved that the Ballot-Papers should be sent for examination at intervals during the time the Ballot remained open.

The Ballot having been declared open, the Secretary read the Annual Report of the Council upon the proceedings of the Institution during the year 1882 and upon its general condition.

Resolved,—That the Report of the Council be received and approved, and that it be printed in the "Minutes of Proceedings" in the usual manner.

The Telford, Watt, and George Stephenson Medals, the Telford and Manby Premiums, and the Miller Scholarships and Prizes, for 1882, which had been awarded, were presented. (Pages 232 to 234.)

Resolved,—That the thanks of the Institution are justly due and are presented to the Vice-Presidents and other Members of Council, for their co-operation with the President, their constant attendance at the Meetings, and their zeal on behalf of the Institution.

Mr. Brunlees, Vice-President, returned thanks.

Resolved unanimously,—That the cordial thanks of the Meeting be given to Sir W. G. Armstrong, President, for his persevering endeavours in the interests of the Institution, for his unremitting attention to the duties of his office, and for the urbanity he has at all times displayed in the Chair.

Sir W. G. Armstrong, President, returned thanks.

Resolved,—That the thanks of the Institution are due and are presented to Messrs. Edward Easton and J. Clarke Hawkshaw, the Auditors appointed at the last Annual Meeting, for the time and trouble they have bestowed in verifying the Accounts; and that Messrs. J. Clarke Hawkshaw and H. M. Brunel be requested to act as Auditors for the ensuing year.

Resolved,—That the cordial thanks of the Institution be tendered to Mr. Charles Manby, the Honorary Secretary, and to Mr. James Forrest, the Secretary, for their long-continued and valued services.

Mr. Forrest returned thanks.

The Scrutineers then announced that the following gentlemen had been duly elected:

President.

JAMES BRUNLEES, F.R.S.E.

Vice-Presidents.

Sir Joseph Wm. Bazalgette, C.B.	Edward Woods.
Sir Frederick J. Bramwell, F.R.S.	George Barclay Bruce.

Other Members of Council.

Benjamin Baker.	Harrison Hayter.
John Wolfe Barry.	William Pole, F.R.SS. L. & E.
George Berkley.	Robert Rawlinson, C.B.
Sir John Coode.	Alexander Meadows Rendel, M.A.
Edward Alfred Cowper.	C. William Siemens, D.C.L., F.R.S.
Sir James Nicholas Douglass.	David Stevenson, F.R.S.E.
Alfred Giles.	Sir W. Thomson, F.R.SS. L. & E.

Sir Jos. Whitworth, Bart., F.R.S.

Resolved,—That the thanks of the Meeting be given to Messrs. Atchison, Blakesley, Chatterton, Dawson, Dobson, Frewer, Gravell, Harris, and Smith, the Scrutineers, for the promptitude and efficiency with which they have performed the duties of their office; and that the Ballot Papers be destroyed.

[ANNUAL REPORT.

ANNUAL REPORT.

In this Report, which the Council has the pleasure to submit to the members on the proceedings of the past year, and on the position attained by the Institution, it will be found that the steady progress which has been a subject for congratulation by successive Councils shows no sign of diminution. The Council has no hesitation in asserting that The Institution of Civil Engineers occupies a higher position now than at any previous period of its existence, as shown by the number of its constituents, the importance of its publications, its relations with kindred associations in all parts of the world, and its financial condition.

The Roll of the Institution.

During the past session 238 candidates have been elected, and 90 names have from various causes disappeared from the register, leaving a net gain of 148, which has increased the aggregate of all classes to 3,385 irrespective of the Students. The details of the several changes are shown in a footnote.[1]

---

[1] The following Table shows the changes that have occurred in the several classes belonging to the Institution, irrespective of the Students, during the last two Sessions:

—	Nov. 30, 1880, to Nov. 30, 1881.						Nov. 30, 1881, to Nov. 30, 1882.					
	Honorary Members.	Members.	Associate Members.	Associates.	Totals.		Honorary Members.	Members.	Associate Members.	Associates.	Totals.	
Numbers at commencement	18	1,209	1,287	568		3,082	18	1,261	1,406	552		3,237
Transferred to Members	..	..	30	2			..	..	36	1		
Do. to Associate Members	..	..	..	1			..	..	..	10		
Elections	..	47	183	7	240		3	48	180	7	238	
Restored to Register	..	1	..	2			..	..	..	..		
Deaths	..	24	24	12	−85		1	29	13	11	−90	
Resignations	..	2	5	8			..	10	4	11		
Erased	..	2	6	2		155	..	..	7	4		148
Numbers at termination	18	1,261	1,406	552		3,237	20	1,307	1,536	522		3,385

The deaths during the year have been:—

Honorary Member: The Rev. Thomas Romney Robinson, D.D., F.R.S.

Members: Charles Edwards Amos (*post*, p. 387)[1]; Thomas Aveling; James Blackburn (lxx. 413); Robert Briggs (*post*, p. 395); Joseph Colthurst; Charles Lennox Davies (*post*, p. 398); John England (lxx. 415); Robert Rowan Greene; Thomas Longridge Gooch; Thomas Richard Guppy (lxix. 411); Alexander Lyman Holley; Henry Hooper (*post*, p. 399); Anthony Henry Kessner (lxx. 415); Stephen William Leach (lxx. 420); Robert Charles May; William Menelaus; Edward Francis Murray; John Murray (*post*, p. 400); Charles George Napier (*post*, p. 407); Cuthbert Knightley Orlebar (*post*, p. 408); Thomas Ormiston (*post*, p. 409); William Powell (*post*, p. 414); Middleton Rayne (*post*, p. 415); Stephen Robinson (lxviii. 312); John Scott Russell, F.R.SS. L. & E.; Carl Louis Schwendler (lxx. 423); George Seaton (lxx. 427); Henry Thomas Tanner (*post*, p. 417); and John Scott Tucker (*post*, p. 418).

Associate Members: James Grey Adamson (lxvii. 403); Henry Alty (*post*, p. 420); Robert Richard Arntz (lxx. 431): Thomas Blair (lxix. 416); Isaac Dodds; Samuel Downing, LL.D.; Henry Garth; Ebenezer Goddard; Edmund Henry Harris; Thomas Milbourne MacFarlaine; Henry Merryweather; James Oliver; and James Richardson.

Associates: John Allison; Henry Crabtree (lxx. 434); William Gilbertson; Lieut.-Colonel William Robert Johnson, M.S.C.; Samuel John Knight (lxx. 434); George Leeman; Lieut.-Colonel Sir William Palliser, C.B., M.P. (lxix. 418); Colonel John Thomas Smith, R.E., F.R.S. (*post*, p. 422); Major William Swainson Suart, R.E. (*post*, p. 424); Charles Tomlison (lxix. 421); and George Vaughan.

The resignations accepted have been—

Members: Thomas Drane; William Henderson; William Luke; Charles Martin; Robert Morgan; George Brice Pennell; Henry Palfrey Stephenson; William Teasdel; Wilberforce Wilson; and Arthur Whitehead.

Associate Members: Robert Carr Harris; Henry Lyon; James Ouchterlony Macdonald; and William Parsey.

Associates: Colonel John Underwood Bateman Champain, R.E.; Henry Slingsby Bethell; William Bevan; Colonel Frederic Brine, R.E., K.T.S.; Henry Carmichael Christopher; Major-General James George Roche Forlong, F.R.S.E.; William Newmarch,

---

[1] The figures in parentheses after some of the names refer to memoirs which have been, or will be, printed in the Minutes of Proceedings.

F.R.S.; Ernest Bengough Ricketts; Henry Rose; Major Hector Tulloch, R.E.; and John Wade.

Of Students 149 have been admitted, and 104 have been removed from the list—of whom 57 have been elected Associate Members, 2 have died, 23 resigned, and 22 were erased—leaving an effective increase of 45, and making the total number now 707.

THE PUBLICATIONS.

Whatever developments the work of the Institution may take in the future, the Minutes of Proceedings must always rank as its most important production, constituting the tangible and useful result of its labours, and the ground upon which its claim is mainly based to the consideration of the profession. The interest of the meetings, at least in their social features, is ephemeral; but the Minutes of Proceedings record the history of civil engineering during the last fifty years. There is one notable distinction between the contents of the early volumes and those at present issued, viz., that the Papers reserved for reading now are of a more debateable character than formerly. Although it cannot be too often remembered that the Institution as a body is not responsible for individual opinions promulgated in the Papers and at the meetings, yet the fact of appearing in the official proceedings of the Society invests such Papers, and their discussion, in the opinion of the public, with an authority they might otherwise lack. It is therefore very desirable that no suspicion of incomplete or one-sided criticism should attach to any of the debates. It is with this view that the "Correspondence" appended to the reports of the discussions has been developed to its present proportions. Originated with the idea of eliciting opinions from persons known to be well acquainted with the subjects brought forward, but who had no opportunity of giving their views orally, the value of this departure from pre-established usage became manifest as the nature of the debates acquired a more controversial character, inevitable when principles are being considered. These remarks must not, however, be taken as implying any depreciation of the descriptions of executed works, which will always retain their value for reference, while Papers involving the consideration of principles may become quite obsolete by reason of the advance of knowledge and of new modes of thought. Although no hard-and-fast line can be drawn, it is now considered more appropriate that accounts of executed works, unless of remarkable novelty and importance, or unless illustrative of principles which will evoke useful discussion, should be inserted in Section II. It is, however, to be noted that in the award of premiums no distinction is made between the two classes of Papers, as may be seen by a list appended to this report.

A short reference to the Papers, read at the twenty-five Ordinary Meetings last session, will serve to illustrate the grounds on which these views are advanced. Mr. Dugald Clerk's memoir on "The Theory of the Gas-Engine" (vol. lxix., p. 220) enunciated opinions which were by some persons warmly assailed. It would have been prejudicial to the Institution, as well as to the Author and his opponents, if any but the widest discussion had taken place, while such ample discussion could only be obtained by inviting persons conversant with the subject but unable to attend to submit their criticisms in writing. The same remarks apply to the Papers of Mr. J. J. Coleman, on "Refrigerating Machinery," and to that of Mr. T. F. Harvey, on "Coal-washing," which, in their consideration of principles, introduced matter that it would have been inexpedient to record without allowing possible critics to urge their views. Both these Papers, as also that of Mr. Proctor Baker, on "Corn-Milling," were of great value and interest, and occupy a high place in the premium list. A Paper by Mr. W. H. Wheeler, on the "Conservancy of Rivers in the Eastern Midland District of England," and another by Mr. Vernon-Harcourt on "Harbours and Estuaries on Sandy Coasts" were in the main descriptive, but afforded abundant material for interesting debate, inasmuch as the former gave instances of the present unsatisfactory condition of many river-basins, while the latter recorded the present condition of nearly twenty first-class coast- or river-harbours, mostly on sandy shores, where the results of different systems of construction could be compared, and their value accurately assessed. The discussion on Mr. Ewing Matheson's Paper on "Steel for Structures" amply supported the Author's views of the great value of this material, the principal point raised being the admissible working stress upon steel. Mr. H. J. Butter's essay on "Forces and Strains of Recoil in the Elastic Field-gun Carriage" was subjected to considerable criticism, for the most part directed against his views of the function of the hydraulic buffer; but high tribute was paid to the value of the facts elicited by the experiments on which the Paper was based. Premiums have been awarded for these eight communications. Here it may be remarked that three out of the eight recipients do not belong to the Institution, for, in accordance with the enlarged views which have obtained for many years, no distinction is drawn between essays received from any one connected with the Society, and those received from any other person, whether a native or a foreigner.

The other Papers discussed at the meetings were by Mr. C. J. Wood on "Iron Permanent Way"; Mr. A. Jacob on "Floods in the Irwell"; Mr. C. B. Bender, and Professor Jules Gaudard, both on "Wind-pressure"; Mr. C. W. Folkard, "The Analysis of Potable Water;" Dr. Paget Higgs, "Candle-power of the Electric Light"; Mr. H. Simon on "Modern Flour Milling in England"; and Mr. W. B. Harding on "Roller-Mills, and Milling as practised at Buda-pest."

In the Papers selected for printing without being publicly read, Mr. J. G. Mair's communication on the "Independent Testing of Steam-engines" described trials of great interest and value to steam-users; but being a record of work done any discussion would probably have taken the form of questions on matters of detail, most of which were anticipated by the full and complete notice of the modes by which the tests were made. Mr. J. Mansergh described the "Lancaster Water Works Extension," his Paper being an excellent account of a constructed work, but of which the interest was mainly local. The same remark applies to the joint Paper by Messrs. Boult and Potts on "The Seacombe Ferry Improvement Works." Mr. C. H. Moberly's account of some "Tests of Riveted Joints," was a valuable contribution to experimental research, and belongs to the same category as Mr. Mair's before mentioned. Mr. R. Harvey's Paper on "Plant for the Manufacture of Iodine," described one of those modern manufacturing processes, carried on wholly by machinery, which necessitates the employment of the engineer, but is of special rather than general interest. Descriptions of "Buckie Harbour" (Mr. James Barron), "Bo'ness Harbour and Docks" (Mr. P. W. Meik), and the "Kawarau Suspension Bridge" (Mr. H. P. Higginson) completed this list of Papers, to which premiums were awarded.

The rules of the Council preclude any awards being made to members of that body, but special thanks were voted to Dr. William Pole, F.R.S., for his contribution on "Aerial Navigation," and to Mr. B. Baker for his Paper on "Steel for Tires and Axles."

Students' meetings were held on every Friday evening from the 13th of January to the end of March, twelve out of fifteen Papers submitted having been considered suitable for reading and discussion. Mr. Alan Brebner, jun., B.Sc., obtained a Miller Scholarship, for his communication on "Dioptric Apparatus," and it, as well as Mr. J. A. Thompson's "On a Screw Tug-Boat," was considered deserving of being printed among the Selected Papers. But the Council regret that, although they were able to confer Miller prizes for seven Papers by Students, there was some falling off in the quality of the communications as compared with previous years. The Miller Prizes have been gained by Messrs. J. A. Thompson (who has previously received a Miller prize), A. H. Case, W. T. Batten, W. Bashall, R. M. Parkinson, L. Samuel, and U. H. Broughton.

The Foreign Abstracts, constituting Section III. of the Proceedings, continue to be thoroughly appreciated by the members, especially by those in India and in the Colonies, whose opportunities are few and scanty of ascertaining the progress of engineering abroad; while foreign engineers frequently and earnestly express their sense of the advantage of thus summarising the current professional literature of all countries.

The present era is one of great activity in all branches of science, and the multiplicity of Associations and Societies puts a severe strain on their officers in the endeavour to obtain good and interesting Papers. Nevertheless, the Council of this Institution ventures to assert that its publications—emanating from a private society, which happily has never received State aid of any kind—must be regarded as highly creditable to the whole body, and that the heavy expense attendant upon the production of these volumes is far more than justified by their enduring value.

In the past year, as in previous years, several applications have been received from Public Offices, Libraries, and Societies for a free gift of the Minutes of Proceedings, or for an exchange of publications. In dealing with such requests the Council is guided partly by the consideration as to whether the publications of the Institution are readily accessible or not in the town or district from whence the request emanates, and partly by the importance of the body making the application.

THE HOUSE OF THE INSTITUTION.

During the recess the premises have been entirely redecorated, new carpets have been supplied, and the furniture has been recovered and re-polished. Ten years having elapsed since similar extensive repairs were undertaken, any further delay seemed undesirable, having regard to the increase in the number of members frequenting the Institution, and to its extended use for the meetings of other Societies. The limit of accommodation in the present premises for the Library and for the offices has however been reached, and it is doubtful whether any modifications of the internal arrangements will suffice to afford the necessary accommodation. In November 1868, when the existing house was first occupied, the total number of members was 1,549, with 133 Students. The gross number has in the interval been considerably more than doubled, while the growth of the correspondence, and, consequently, of the documents to be kept, may be shown by the fact that at the date given, a 1000-page letter-book sufficed for nine months, whereas it now lasts but six weeks. Another pressing want is further shelf-room for the books in the Library. Since 1868 the shelves have been added to on three separate occasions, with the result that all the space then provided for the future has been absorbed, and now the passages and lobbies in the upper part of the house have had to be used. By placing on the shelves thus provided the books in least demand, the inconvenience attending the scattered accommodation has been thus far lessened; but it is still felt, and of course the books cannot be so well preserved as they would be in a room.

The following Societies have been granted the gratuitous use of the premises for holding their meetings, namely, the Institution of Mechanical Engineers, the Iron and Steel Institute, the Society of Telegraph Engineers and of Electricians, the Meteorological Society, the Society of Municipal and Sanitary Engineers and Surveyors, and the Gas Institute, all of which have passed resolutions acknowledging in appropriate terms the facilities afforded to them.

The collection of portraits of eminent engineers has been enriched by a gift from Mr. Charles Greaves, Member, of a portrait in oil of Sir Hugh Myddelton, accompanied with an engraving of the same.

THE LIBRARY.

The books in the Library have recently been compared with the catalogue, and every work was found to be properly entered. Where additions to serial publications had not been received for some time the missing parts were written for, and were usually obtained. The total number of volumes in the Library is 18,302. Of these, 345 are volumes of tracts, each containing on an average about fifteen pamphlets. The serial publications are 349 in number.

A retrospect showing the growth of the Library may not prove uninteresting. The first catalogue was printed in 1850, at which time the Library contained about 3,300 volumes; in 1865, when a new catalogue was issued, the number had grown to 5,500, showing an average increase of 140 a year. At the commencement of 1880 the books were again counted, and the number proved to be 15,900. The present annual increase is about 800 volumes. Among the additions this year there have been some exceedingly valuable donations, notably a complete collection of Official Reports on Italian Public Works presented by the Italian Government; a series of lectures delivered at the École des Ponts et Chaussées, Paris, obtained through the courtesy of the authorities of the school; a set of the most recently published papers on Belgian State Railways and Inland Navigation received from the Belgian Government; and a number of American State documents. While on this subject the Council must again request members not to forget their obligations to the Institution. Whether the collection of books now within these walls is in the future to keep pace with the progress of the profession must depend in no small measure on the assistance rendered by the members.

FINANCE.

The Council can refer with satisfaction to the financial condition of the Institution. The income for the year ended on the 30th ult. amounted to £12,898 10*s.* 11*d.*; whilst the capital and Trust fund receipts were £3,527 9*s.* 6*d.* and £431 15*s.* 6*d.* On the other hand the general expenditure reached £12,788 5*s.* 7*d.* This amount includes £1,500 on account of the repairs and decoration of the premises; as well as £5,806 6*s.* 3*d.* the cost of producing the four volumes of Minutes of Proceedings. In respect of Capital, £3,000 London and North Western Railway Four per Cent. Debenture Stock was purchased at a cost of £3,380 13*s.* 6*d.*, while a sum of £497 3*s.* 4*d.* was expended under Trusts, one hundred guineas of this being for new dies for the Telford medal, cracks having gradually developed themselves in the old dies from long-continued use.

In pursuance of an alternative obligation entered into on election, 87 members of different classes have together contributed £252 3*s.* 9*d.* to the Library Fund. A list of these donors is appended to this Report. Three members have shown their appreciation of the publications by voluntarily adding one guinea each to their ordinary subscriptions.

The Council has recently received a communication which recalls the memory of one of the founders of the Institution.

This gentleman, Mr. Henry Robinson Palmer, was well known to the last generation of engineers as Telford's chief assistant, and as a Vice-President of this Society. He died in the prime of life in 1844, but his widow survived until this year. Desiring that the honourable connection of her husband with the profession should be perpetuated, she, by a memorandum attached to her will, directed that a sum of £1,500 (less legacy duty) should, on the expiry of a life interest in it, be devoted to found a Scholarship at Cambridge University, tenable by the son of a Civil Engineer who needed the help, the holder to be from time to time nominated by the Council of the Institution. The present Council, for itself and its successors, has accepted the Trust.

Looking to the number of members in the Institution, and particularly to the large proportion of young life—looking also to its accumulated means and its satisfactory income, the Council entertains the confident belief that the Institution must advance in importance and utility, if only the members continue to take the same active interest in the welfare in the future as their predecessors have done in the past.

ABSTRACT *of* RECEIPTS *and* EXPENDITURE

RECEIPTS.

Dr.	£.	s.	d.	£.	s.	d.
To balance in the hands of the Treasurer	750	7	11			
„ „ Secretary	21	4	2	771	12	1

INCOME.

	£.	s.	d.	£.	s.	d.
— Subscriptions:—						
Arrears	262	11	0			
Current	10,498	5	6			
Advance	113	13	6			
— Library Fund	252	3	9			
— Minutes of Proceedings:—Repayment for Binding, &c.	210	13	11			
— Publication Fund	3	3	0			

— Dividends: 1 year on

£.	s.	d.	*Institution Investments.*	£.	s.	d.
4,750	0	0	Great Eastern Railway Four per Cent. Debenture Stock . .	186	0	10
3,000	0	0	London and North Western Ditto	117	10	0
1,500	0	0	London, Brighton, and South Coast Ditto	58	15	0
3,000	0	0	North Eastern Ditto . . .	117	10	0
3,000	0	0	Great Northern Ditto . . .	117	10	0
3,000	0	0	Lancashire and Yorkshire Ditto	117	10	0
3,000	0	0	Great Western Ditto . . .	117	10	0
3,000	0	0	Caledonian Ditto	117	0	11
3,000	0	0	Midland Ditto	117	10	0
3,000	0	0	Highland Ditto	117	1	8
1,500	0	0	London, Brighton, and South Coast Railway Four and a Half per Cent. Ditto . . .	66	1	10
3,000	0	0	Manchester, Sheffield, and Lincolnshire Ditto	132	3	8
2,088	11	8	New Three per Cents. . . .	61	3	1
			6 months on			
3,000	0	0	London and North Western Railway Four per Cent. Debenture Stock	58	15	0
£39,838	11	8	Total nominal or par value.			

	£.	s.	d.	£.	s.	d.
— Interest on Deposit Account	52	16	11			
— Sale of Old Materials	3	1	4	12,898	10	11

CAPITAL.

	£.	s.	d.	£.	s.	d.
— Admission Fees	2,576	14	0			
— Life Compositions	950	15	6	3,527	9	6
Carried forward				£17,197	12	6

from the 1ST DEC., 1881, *to the* 30TH NOV., 1882.

PAYMENTS.

GENERAL EXPENDITURE.

Cr.	£.	*s.*	*d.*	£.	*s.*	*d.*
By House and Establishment Charges:—						
Repairs, General	64	16	5			
Decoration of Premises	1,500	0	0			
Rent	644	12	10			
Rates and Taxes	362	8	10			
Rent of Telephone Line and Instruments	23	13	0			
Insurance	41	16	6			
Fixtures and Furniture	200	12	7			
Lighting and Warming	95	2	11			
Tea, Coffee, &c.	56	5	4			
Assistance at Meetings	31	4	0			
Household Expenses	111	10	2			
				3,132	2	7
— Postage, Telegrams, and Parcels	191	2	4			
— Stationery and Printing	496	0	8			
— Watt Medal	2	7	6			
— George Stephenson Medals	7	2	6			
— Diplomas	21	15	6			
— Annual Dinner (Official Invitations, &c.)	143	8	6			
				861	17	0
— Salaries	1,900	0	0			
— Clerks, Messengers, and Housekeeper	636	18	4			
— Donation to late Housekeeper	30	0	0			
				2,566	18	4
— Library:—						
Books	284	17	6			
Periodicals	22	3	2			
Binding	114	0	9			
				421	1	5
— Publication:—						
"Minutes of Proceedings" (Vols. lxvii., lxviii., lxix., and lxx.)				5,806	6	3
				12,788	5	7

CAPITAL INVESTMENT.

	£.	*s.*	*d.*
— £3,000 London and North Western Railway Four per Cent. Debenture Stock	3,380	13	6
Carried forward	£16,168	19	1

ABSTRACT *of* RECEIPTS *and* EXPENDITURE

RECEIPTS—*continued.*

Dr.									£.	s.	d.
				Brought forward					17,197	12	6
				TRUST FUNDS.							
To Dividends: 1 year on											
	£.	s.	d.	*Telford Fund.*	£.	s.	d.				
	2,839	10	10	Three per Cent. Consols . .	83	8	0				
	2,586	0	11	Three per Cent. Reduced . .	75	14	4				
	2,377	10	5	Three per Cent. Consols (Unexpended Dividends) . . .	69	17	0				
	913	2	7	Three per Cent. Reduced (Ditto)	26	14	10				
	8,716	4	9	Total nominal or par value.							
				Manby Donation.							
	250	0	0	Great Eastern Railway Four per Cent. Debenture Stock . .	9	15	10				
				Miller Fund.							
	3,125	0	0	New Three per Cents. . . .	91	10	1				
	643	19	8	Three per Cent. Consols (Unexpended Dividends) . . .	18	18	4				
	1,355	14	11	Three per Cent. Reduced (Ditto)	39	13	11				
	5,124	14	7	Total nominal or par value.							
				Howard Bequest.							
	551	14	6	New Three per Cents. . . .	16	3	2				
									431	15	6
									£17,629	8	0

SUMMARY OF INVESTMENTS.

	£.	s.	d.	£.	s.	d.
INSTITUTION INVESTMENTS				39,838	11	8
TRUST FUNDS—						
Telford Fund	8,716	4	9			
Manby Donation	250	0	0			
Miller Fund	5,124	14	7			
Howard Bequest	551	14	6			
				14,642	13	10
Total nominal or par value . . .				£54,481	5	6

from the 1ST DEC., 1881, *to the* 30TH NOV., 1882.

PAYMENTS—*continued.*

Cr.

	£.	s.	d.	£.	s.	d.
Brought forward				16,168	19	1
TRUST FUNDS.						
By Telford Premiums	237	9	4			
— „ Medal—New Dies	105	0	0			
— Miller Scholarships	40	0	0			
— Miller Prizes	114	14	0			
				497	3	4
				16,666	2	5
— Balance Nov. 30, 1882, viz.:—						
Cash in the hands of the Treasurer	947	0	9			
„ „ Secretary	16	4	10			
				963	5	7
				£17,629	8	0

Examined with the Books and Securities and found correct.

(Signed) EDWARD EASTON, J. CLARKE HAWKSHAW } *Auditors.*

JAMES FORREST, *Secretary,*
16*th December,* 1882.

PREMIUMS AWARDED.

SESSION 1881–82.

THE COUNCIL of The Institution of Civil Engineers have awarded the following Premiums:

FOR PAPERS READ AT THE ORDINARY MEETINGS.

1. A Watt Medal and a Telford Premium to Dugald Clerk, for his Paper on "The Theory of the Gas-Engine."

2. A Watt Medal and a Telford Premium to Joseph James Coleman, for his Paper on "Air-Refrigerating Machinery and its Applications."

3. A George Stephenson Medal and a Telford Premium to Thomas Fletcher Harvey, Assoc. M. Inst. C.E., for his Paper on "Coal-Washing."

4. A Watt Medal and a Telford Premium to William Proctor Baker, for his Paper "On the Various Systems of Grinding Wheat, and on the Machines used in Corn-Mills."

5. A Telford Premium to William Henry Wheeler,[1] M. Inst. C.E., for his Paper on "The Conservancy of Rivers: the Eastern Midland District of England."

6. A Telford Premium to Leveson Francis Vernon-Harcourt,[2] M.A., M. Inst. C.E., for his Paper on "Harbours and Estuaries on Sandy Coasts."

7. A Telford Premium to Ewing Matheson, M. Inst. C.E., for his Paper on "Steel for Structures."

8. The Manby Premium to Henry Joseph Butter, M. Inst. C.E., for his Paper on "Forces and Strains of Recoil considered with reference to the Elastic Field Gun-Carriage."

---

[1] Has previously received a Telford premium.
[2] Has previously received the Manby premium.

For Papers Printed in the Proceedings without being Discussed.

1. A Watt Medal and a Telford Premium to John George Mair, M. Inst. C.E., for his Paper "On the Independent Testing of Steam-Engines, and the Measurement of Heat used."

2. A Telford Medal and a Telford Premium to James Mansergh, M. Inst. C.E., for his Paper on "The Lancaster Waterworks Extension."

3. A Telford Medal and a Telford Premium to Wilfrid Swanwick Boult, Assoc. M. Inst. C.E., and a Telford Medal and a Telford Premium to John James Potts, Assoc. M. Inst. C.E., for their joint Paper on the "Seacombe Ferry Improvement-Works."

4. A Telford Premium to Charles Henry Moberly, M. Inst. C.E., for his "Account of some Tests of Riveted Joints for Boilerwork."

5. A Telford Premium to Robert Harvey, Assoc. M. Inst. C.E., for his Paper on "Plant for the Manufacture of Iodine."

6. A Telford Premium to James Barron, Assoc. M. Inst. C.E., for his Paper on "Buckie Harbour."

7. A Telford Premium to Patrick Walter Meik, M. Inst. C.E., for his Paper on "Bo'ness Harbour and Dock-works."

8. A Telford Premium to Harry Pasley Higginson, M. Inst. C.E., for his Paper on "The Kawarau Suspension Bridge, N.Z."

The special thanks of the Council were voted to their colleagues Dr. William Pole, F.R.S., and Mr. B. Baker, for their contributions on "Aerial Navigation," and on "Steel for Tires and Axles."

FOR PAPERS READ AT THE SUPPLEMENTAL MEETINGS OF STUDENTS.

1. The Miller Scholarship to Alan Brebner, jun., B.Sc., Stud. Inst. C.E., for his Paper on "Dioptric Apparatus in Lighthouses."

2. A Miller Prize to John Augustus Thompson,[1] Stud. Inst. C.E., for his "Description of a Composite Screw Tug-Boat."

3. A Miller Prize to Albert Havelock Case, Stud. Inst. C.E., for his Paper on "Cranes and Lifting-Apparatus."

4. A Miller Prize to William Townshend Batten, Stud. Inst. C.E., for his Paper on "Modern Apparatus for the Manufacture and Purification of Coal-Gas."

5. A Miller Prize to William Bashall, Stud. Inst. C.E., for his Paper on "Laboratory Work: Iron and Steel in Compression, Hardened Iron in Tension and Deflection."

6. A Miller Prize to Richard Marion Parkinson, Stud. Inst. C.E., for his Paper on "The Swindon, Marlborough, and Andover Railway."

7. A Miller Prize to Louis Samuel, Stud. Inst. C.E., for his Paper on "Excavating- and Dredging-Plant."

8. A Miller Prize to Urban Hanlon Broughton, Stud. Inst. C.E., for his Paper on the "Narrow-Gauge Railways of Ireland."

---

[1] Has previously received a Miller prize.

SUBJECTS FOR PAPERS.

Session 1882–83.

The Council of The Institution of Civil Engineers invite communications, of a complete and comprehensive character, on any of the Subjects included in the following list, as well as on other analogous questions. For approved Original Communications, the Council will award Premiums, arising out of Special Funds bequeathed for the purpose, the particulars of which are as under:—

1. The Telford Fund, left "in trust, the Interest to be expended in Annual Premiums, under the direction of the Council." This bequest (with accumulations of dividends) produces £260 annually.

2. The Manby Donation, of the value of about £10 a year, given "to form a Fund for an Annual Premium or Premiums for Papers read at the meetings."

3. The Miller Fund, bequeathed by the testator "for the purpose of forming a Fund for providing Premiums or Prizes for the Students of the said Institution, upon the principle of the 'Telford Fund.'" This Fund (with accumulations of dividends) realises £160 per annum. Out of this Fund the Council have established a Scholarship,—called "The Miller Scholarship of The Institution of Civil Engineers,"—and are prepared to award one such Scholarship, not exceeding £40 in value, each year, and tenable for three years.

4. The Howard Bequest, directed by the testator to be applied "for the purpose of presenting periodically a Prize or Medal to the author of a treatise on any of the Uses or Properties of Iron, or to the inventor of some new and valuable process relating thereto, such author or inventor being a Member, Graduate, or Associate of the said Institution." The annual income amounts to rather more than £16. It has been arranged to award this prize every five years, commencing from 1877. The next award will therefore be made in 1887.

The Council will not make any award unless a communication of adequate merit is received, but will give more than one Premium if there are several deserving memoirs on the same subject. In the adjudication of the Premiums no distinction will be made between essays received from any one connected with the Institution (except in the cases of the Miller and the Howard bequests, which are limited by the donors), or from any other person, whether a Native or a Foreigner.

List.

1. The recent development in the use of Graphic Methods for the solution of Engineering Problems.
2. The more complete determination of Coefficients of Friction, especially as applied to Friction-brakes.
3. The Measurement of Work by Dynamometers and similar machines.
4. The effect produced on the Mechanical and other Properties of Steel by tempering in oil and in water.
5. The Methods of protecting Metal-work exposed to Corrosion, with examples.
6. The Productive Power of Machine-Tools, and the modes of increasing it.
7. The application of Hydraulic Pressure in the construction of Ironwork.
8. Stamping and Welding under the Steam-hammer, and by Hydraulic Pressure.
9. The means of producing and applying cheap Gas for Motive-power.
10. The type of Steam-Engine best adapted for ordinary factory purposes, in respect to economy in first cost, and in cost of working and maintenance.
11. Caloric, or Hot-air, Engines.
12. Drilling, Riveting, and other Machinery for Girder-work.
13. The Removing of Buildings bodily, as practised in the United States.
14. Mechanical power on Tramways, including Steam, Compressed-air, Electricity, Cables, &c.
15. Railway construction in the United States and in Canada.

16. Maritime Canals and Ship-Railways.

17. The Design and Construction of Slip-ways for Repairing Ships.

18. The difference in design of British and Foreign Locomotive Engines.

19. The Application of the Compound Principle to Locomotive Engines.

20. The latest development of the Compound Marine-Engine.

21. Vessels for Inland Navigation, with the mode of working them by stern-wheels, screw-propellers, &c.

22. The Comparative Cost of Transport by Land and Water.

23. The Utilisation of the Mechanical Power of the Tide and of the Current of Rivers.

24. A Review of recent Hydraulic Experiments, with a description of the Floats and Current-Meters employed for determining the Discharge of Rivers.

25. The Works carried out on the Continent of Europe and in North America for the Improvement of Rivers, and of Inland Navigation generally.

26. The Design and Construction of Covered Service-Reservoirs for Town Water-Supply.

27. The Sewering of Towns on the separate system.

28. The Methods and Appliances for Blasting Rock under Water.

29. The Transportation, Storage, and Shipment of Grain.

30. Ice-Storing, with Description of Buildings and Appliances.

31. The Distribution of Electricity over large areas, for lighting and for motive-power.

32. Electric Railways.

33. The Methods employed in securing large and irregular-shaped mineral workings, for example—the Almaden Mines, the Great Comstock Lode, &c.

34. The Manufacture of Lead and the Extraction of Silver.

35. The Distribution of Heat over large areas from a central source of supply.

36. The Methods and Machinery employed for separating the impurities from coal as carried out in South Wales, in connection with the manufacture of coke for the iron and steel trades.

37. Coal-Depôts for Ocean Steamers, the various points involved in their management, and the preservation of the coal from deterioration.

38. Large-bore Naval and Coast-battery Ordnance, and the form of projectile best adapted for range, penetrative power and general useful effect.

INSTRUCTIONS FOR PREPARING COMMUNICATIONS.

The Essays should be written in the third person, and be legibly transcribed on foolscap paper, on one side only, leaving a margin on the left side, in order that the sheets may be bound. Every Paper must be prefaced by an Abstract not exceeding 1500 words in length.

Illustrations, when necessary, should be drawn on tracing-paper, to as small a scale as is consistent with distinctness, and ready to be engraved. When an illustrated communication is accepted for reading, a series of Diagrams will be required sufficiently large and boldly coloured to be clearly visible at a distance of 60 feet. These diagrams will be returned.

Papers which have been read at the Meetings of other Societies, or have been published in any form, cannot be read at a Meeting of the Institution, nor be admitted in competition for the Premiums.

The Communications must be forwarded to the Secretary of the Institution, from whom any further information may be obtained. There is no specified date for the delivery of MSS., as when a Paper is not in time for one session it is dealt with in the succeeding one.

CHARLES MANBY, *Honorary Secretary.*
JAMES FORREST, *Secretary.*

THE INSTITUTION OF CIVIL ENGINEERS,
25, *Great George Street, Westminster, S.W.*
1*st November*, 1882.

EXCERPT BY-LAWS, SECTION XV., CLAUSE 3.

"Every Paper, Map, Plan, Drawing, or Model, presented to the Institution, shall be considered the property thereof, unless there shall have been some previous arrangement to the contrary, and the Council may publish the same in any way and at any time they may think proper. But should the Council refuse or delay the publication of such Paper beyond a reasonable time, the Author thereof shall have a right to copy the same, and to publish it as he may think fit, having previously given notice, in writing, to the Secretary of his intention. Except as hereinbefore provided, no person shall publish, or give his consent for the publication of any communication presented and belonging to the Institution, without the previous consent of the Council."

NOTICE.

It has frequently occurred that in Papers which have been considered deserving of being read and published, and have even had Premiums awarded to them, the Authors may have advanced somewhat doubtful theories, or may have arrived at conclusions at variance with received opinions. The Council would therefore emphatically repeat, that the Institution as a body must not be considered responsible for the facts and opinions advanced in the Papers or in the consequent Discussions; and it must be understood, that such Papers may have Medals and Premiums awarded to them, on account of the Science, Talent, or Industry displayed in the consideration of the subject, and for the good which may be expected to result from the inquiry; but that such notice, or award, must not be regarded as an expression of opinion, on the part of the Institution, of the correctness of any of the views entertained by the Authors of the Papers.

ORIGINAL COMMUNICATIONS

RECEIVED BETWEEN DECEMBER 1, 1881, AND NOVEMBER 30, 1882.

AUTHORS.

Addy, J. No. 1,895.—The Water Supply of Peterborough. With Appendix and 6 Diagrams.

Baker, B. No. 1,838.—Steel for Tires and Axles. (Vol. lxvii., p. 353.)

——, W. P. No. 1,879.—On the Various Systems of Grinding Wheat, and on the Machines used in Corn-Mills. (Vol. lxx., p. 162.)

Barron, J. No. 1,861.—Buckie Harbour. With 1 Drawing. (Vol. lxx., p. 350.)

Bell, C. N. No. 1,897.—The Sewerage and Drainage of Christchurch, New Zealand. With 2 sheets of Illustrations.

Bell-Irving, H. O. No. 1,873.—Weights of Double-line Railway Plate-Girder Bridges, and Loads on them. With 2 sheets of Illustrations.

Blum, R. No. 1,843.—The Current-Meter of Professor A. R. Harlacher. With Illustrations. (Vol. lxvii., p. 358.)

Botley, C. E. No. 1,885.—Drawing of an Original Design for the Manufacture of Sulphate of Ammonia from Gas Liquor.

Briggs, R. (the late). No. 1,842.—American Practice in Heating Buildings by Steam. (*Ante*, p. 95.)

Brunton, J. No. 1,818.—The Oxford and District Tramways. With 4 Illustrations. (Vol. lxvii., p. 345.)

Buck, J. H. W. No. 1,894.—Weights of Girders Determined Graphically. With 4 Figs. and 2 Diagrams.

Burke, C. T. No. 1,892.—The Ashti Tank. With 2 Photographs and 12 sheets of Illustrations.

Clerk, D. No. 1,855.—The Theory of the Gas Engine. With 17 Illustrations. (Vol. lxix., p. 220.)

Coleman, J. J. No. 1,845.— Air-Refrigerating Machinery and its Applications. With 3 Plates. (Vol. lxviii., p. 146.)

Collins, H. R. No. 1,871.—Tramways in Natal.

Cottrell, H. E. P. No. 1,890.—On the Improved Construction and Use of the Plane Table. With 1 sheet of Illustrations.

Cunningham, Major A., R.E. No. 1,876.—Recent Hydraulic Experiments. (*Ante*, p. 1.)

AUTHORS.

Daglish, J. No. 1,866.—On the Sinking of two Shafts at Marsden. for the Whitburn Coal Company. With 8 Diagrams. (*Ante*, p. 178.)

Darwin, G. H. No. 1,904.—On the Horizontal Thrust of Sand. With 11 Diagrams. (*Post*, p. 350.)

Duckham, F. E. No. 1,860.—Grain Depôt and Appliances at the Millwall Docks. With 1 sheet of Illustrations, Map and Specification.

Fahey, C. S. No. 1,875.—The River Indus as a Source of Supply for Irrigation-Canals in Sind. (*Post*, p. 279.)

Fernie, J. No. 1,869.—Mild Steel for Fire-boxes of Locomotive Engines in the United States of America. With 5 Diagrams and specimens of Steel.

Folkard, C. W. No. 1,837.—The Analysis of Potable Water, with special reference to the determination of Previous Sewage Contamination. (Vol. lxviii., p. 57.)

Frecheville, R. J. No. 1,893.—The Mining and Treatment of Gold Ores in the North of Japan. With 5 sheets of Illustrations.

Gobert, A. No. 1,868.—Canal-Navigation in Belgium. (Vol. lxviii., p. 278.)

Harding, W. B. No. 1,877.—Roller-Mills and Milling as practised in Budapest. (Vol. lxx., p. 234.)

Harvey, R. No. 1,850.—Plant for the Manufacture of Iodine. (Vol. lxix., p. 320.)

———, T. F. No. 1,870.—Coal-Washing. (Vol. lxx., p. 106.)

Higgs, P. No. 1,839.—Candle-Power of the Electric Light. (Vol. lxviii., p. 117.)

Jacob, A. No. 1,835.—The Conservancy of Rivers: the Valley of the Irwell. (Vol. lxvii., p. 233.)

Kühl, C. H. L. No. 1,841.—Breaking up a Wreck by Dynamite on the Lower Danube. (Vol. lxix., p. 308.)

Lightfoot, T. B. No. 1,878.—The Design and Construction of Repairing-Slipways for Ships.

Mair, J. G. No. 1,872.—On the Independent Testing of Steam-Engines and the Measurement of Heat used. With Appendix, Tables, and Illustrations. (Vol. lxx., p. 313.)

Mann, I. J. No. 1,844.—On the Adhesive Strength of Portland Cement; with special reference to an improved method of Testing that material. (*Post*, p. 251.)

Matheson, E. No. 1,851.—Steel for Structures. (Vol. lxix., p. 1.)

AUTHORS.

Margary, P. J. No. 1,848.—Reconstruction of the St. Pinnock and Moorswater Viaducts on the Cornwall Railway. (Vol. lxix., p. 312.)

Meik, P. W. No. 1,865.—Bo'ness Harbour and Dock-works. (Vol. lxx., p. 361.)

Moberly, C. H. No. 1,856.—Account of some Tests of Riveted Joints for Boiler-work. (Vol. lxix., p. 337.)

More, C. J. No. 1,901.—Boat-Launches on the River Thames. With 1 Drawing.

Morris, W. No. 1,889. Description of Covered Service-Reservoirs. With 25 sheets of Illustrations.

Mosse, J. R. No. 1,891.—The Restoration of Ancient Tanks in Ceylon. With 34 Drawings.

O'Meara, P. No. 1,902.—The Introduction of Irrigation in New Countries, as illustrated in North Eastern Colorado. With 6 Photographs, 5 Diagrams, and 3 Maps.

Pole, W. No. 1,840.—A Study of the Problem of Aerial Navigation, as affected by Recent Mechanical Improvements. With 2 Wood-cuts. (Vol. lxvii., p. 369.)

Pownall, C. A. W. No. 1,862.—Earthwork Slips on the *Rugby* and Northampton Branch of the London and North Western Railway. With 2 Drawings.

Quigly. R. No. 1,888.—Description of an Improved Governor for Marine (Screw) Engines. With 1 sheet of Illustrations.

Ridings, H. S. No. 1,887.—The Mountain-Railways of Southern Peru.

Ridley, W. No. 1,903.—On the Weirs across the Rivers Koondair and Pennair, Madras Irrigation and Canal-works. With 5 sheets of Illustrations.

Rippl, W. No. 1,864.—The Capacity of Storage Reservoirs for Water Supply according to the English System. (*Post*, p. 276.)

Robertson, F. E. No. 1,886.—An Account of the Indus Valley (State) Railway Ferry over the River Indus at Sukkur. With 1 sheet of Illustrations.

Saldini, Professor C. No. 1,896. — Modern Milling. (*Post*, p. 334.)

Shaw, H. S. H. No. 1,867.—The Measurement of Velocity for Engineering Purposes. (Vol. lxix.,p. 364.)

Simon, H. No. 1,874.—Modern Flour-Milling in England. (Vol. lxx., p. 191.)

AUTHORS.

Slagg, C. No. 1,884.—The Burning of Town-Refuse at Leeds. (Vol. lxviii., p. 290.)

Smith, W. No. 1,899.—Summit-Level Tunnel on the Bettws and Festiniog Railway.

Trewby, G. C. No. 1,849.—System of Unloading and Storing Coals at the Beckton Station of the Gas Light and Coke Company. (Vol. lxix., p. 318.)

Tufnell, C. F. No. 1,852. Economical River-Training in Northern India. With 1 Drawing.

Tweddell, R. H. No. 1,880.—On the Productive Power and Efficiency of Machine-Tools, and on Labour-Saving Appliances worked by Hydraulic Pressure.

Unwin, W. C. No. 1,900.—Current-Meter Observations in the Thames. (*Post*, p. 338.)

Wex, G. Ritter von. No. 1,854.—The Regulation of Rivers and Waterways, with a view to the Prevention of Floods. (Vol. lxix., p. 323.)

Weyrauch, Professor Dr. J. No. 1,882.—Various Methods of Determining Dimensions of Iron Structures. (*Post*, p. 298.)

Williams, E. L. No. 1,881. On the Recent Landslips in the Salt Districts of Cheshire. (Vol. lxx., p. 378.)

LIST OF DONORS TO THE LIBRARY.

From December 1, 1881, to November 30, 1882.

A.

Achard, A.
Acland, H. W.
Aguillon, L.
Akerman, R.
Alexandre, M.
Allan, J.
Allan, J. F.
Allard, M. E.
Almontéo, H. E. Col. J.
Ansley, G. D.
Arbel, A.
Artingstall, S. G.
Ashbury, T.
Atkinson, R.
Attwood, G.

B.

Bagot, A.
Ballard, R.
Bancroft, H.
Barlow, P. W.
Barnet, S., jun.
Barreau, M.
Barry, J. W.
Basu, F. M.
Bateman, J. F.
Bauerman, H.
Baum, C.
Bazalgette, Sir J. W.
Bellingham, W.
Beloe, C. H.
Benét, Maj.-Gen. S. V., U.S.A.
Berby, J. A.
Bergeron, C.
Berrier-Fontaine, Marc.
Bigg, R. H.
Blackett, J.
Blake, J. H.
Bonolis, A.
Boulton, S. B.
Bourne, J.
Bovey, Prof. H. T.
Braet, G.
Bramwell, Sir F. J.
Brandão, Lieut. F. de P. B.
Breary, F. W.
Brereton, R. M.
Britten, B.
Brixby, Lieut. W. H., U.S.A.
Bromley, W. B.
Brown, O.
Buelna, E.
Bunte, Dr. H.
Burke, C. T.
Bylandt, H. E. Count C. de.

C.

Cadart, A.
Caland, P.
Campbell, F. A.
Campbell, J. D.
Carletti, C.
Carpmael, C.
Cates, A.
Chalmers, P.
Chaney, H. J.
Channel Tunnel Co.
Chardonnet, M. le
Chastelnau, C.
Chatterton, G.
Chauvinière, Capt. L. de la
Christie, W. H. M.
Churruca, E. de
Cialdi, A.
Clark, E.
Clark, L.
Clark, Standfield & Co., Messrs.
Clarke, H.
Clarke, T. C.
Clausius, R.
Clericetti, Prof. C.
Colladon, C.
Common, A. A.
Corcoran, A. J.
Cornaglia, P. A.
Cossoux, N. V. L.
Cottrau, A.
Couchman, Hon. T.
Crellin, W.
Cruls, L.
Culcheth, W. W.

D.

Danvers, J.
Day, St. J. V.
Deacon, G. F.
Deas, J.

Demoly, A.
Denny, W.
Denton, J. B.
De Rance, C. E.
Dornfeld, R.
Douglass, Sir J. N.
Dredge, J.
Dyckerhoff, R.

E.

Eads, J. B.
Egleston, Prof. T.
Ebb, O.
Ellacott, John
Emden, A.
Evans, Dr. J.
Evans, W. W.

F.

Fanning, Col. J. T., U.S.A.
Favaro, Prof. A.
Fink, A.
Fletcher, L. E.
Fogerty, J.
Forney, M. N.
Foster, C. le N.
Fowler & Baker, Messrs.
Fowler, R.
Fränkel, Dr. W.
Freshfield & Williams, Messrs.
Fulton, H. H.

G.

Gamble, J. G.
Gizycki, Dr. von
Goodall, J.
Goodwin, H. S.
Greaves, C.
Greenhill, Prof. A. G.
Grey, H.
Griffith, J. P.
Griggs, W.
Grimshaw, R.
Gros, M.
Grover, J. W.
Grüner, L.

H.

Hagen, G.
Hallauer, O.
Harlacher, Prof. A. R.
Harrison, J. T.
Hartley, F. W.
Hawkshaw, J. C.
Hayter, Hon. H. H.
Haywood, Col. W.
Hazen, Maj.-Gen. W.B., U.S.A.
Heaphy, M.
Heath, F. A.
Hedger, P.
Hedges, K.
Hering, R.
Hersent, H.
Hill, J. W.
Hind, J. R.
Hirn, G. A.
Hodgetts, J. F.
Hodson, G.
Hogg, H. J. U.
Holtham, E. G.
Howard, W. F.
Huët, A.
Hull, Prof. E.
Humphreys, Major-Gen. A.A., U.S.A.
Hunt, A. R.
Hunt, Hon. W. H.
Hunter, R.
Huxham, H.

I.

Isherwood, F. B.
Ishiguro, I.
Italian Minister of Public Works, H.E. the.

J.

Jamieson, A.
Japanese Minister, H.E. the.
Jeans, J. S.
Jesse, G. R.
Jinman, G.
Jocelyn, Hon. W. N.
Jochams, F.
Johnson, J.
Johnson, S. W.
Jordan, C. H.

K.

Kapp, G.
Kato, H.
Keller, H.
Keller, Prof. K.
Kennedy, J.
Ketchum, H. G. C.
King, S. P.
Kirk, A. C.
Kirtley, W.
Kock, F.
Köpcke, C.
Kraft, J.
Kühl, C. H. L.

L.

Laidley, Col. F. T. S., U.S.A.
Lalanne, L.
Lanfranconi, G. E.
Latimer, C.
Lavoinne, E.
Law, H.
Leber, M. E. von
Lesseps, C. A. de
Longridge, J. A.

Longridge, M.
Lovegrove, J.
Lynde, J. G.
Lyster, G. F.

M.

M^c^Cosh, J.
Mackesy, Lieut.-Col. W. H.
Mackie, H. U.
McLeod, D. A.
Macleod, K. M.
Macnamara, W. H.
Maere-Limnander, A. de
Maineri, Prof. B. E.
Mair, J. G.
Mallet, Prof. J. W.
Mallory, Col. W. H., U.S.A.
Malo, L.
Manby, C.
Manlove, Alliott, Fryer & Co., Messrs.
Mansell, R.
Manson, G.
Marsaut, J. B.
Mattos, A. M. de
Maxwell-Lyte, F.
Melsens, M.
Menabrea, H.E. Gen. Count.
Merchant, S. L.
Merrill, Col. W. E., U.S.A.
Meunier, W.
Meyer, G.
Mignon & Rouart, Messrs.
Millar, W. J.
Molesworth, G. L.
Monson, E.
Mosse, J. R.
Müller, Dr. F. C. G.

N.

Newall, R. S.
Newbigging, T.
Nimmo, Hon. J., Jun.
Nördling, W. von.
North, F. W.

O.

Oliver, E. E.
Olsen, H. B.
O'Meara, P.
O'Neill, C.
Owen, G. W.

P.

Pain, C. S.
Parker, W.
Parnell, Maj. A., R.E.
Patterson, C. P.
Pearce, W.
Pearse, J. W.
Perissé, S.
Perrier, R.
Perry, Rev. S. J.
Petit, Lieut. W.
Phelps, S. L.
Pidgeon, D.
Pilbrow, J.
Plenkner, W.
Pole, Dr. W.
Pontzen, E.
Potts, B. L. F.
Preston, Rev. T. A.
Prindle, F. C.
Proell, Dr. R.

Q.

Quinette de Rochemont, Baron.

R.

Ravel, J.
Rawlinson, R.
Reade, T. M.
Redman, J. B.
Reitz, T. H.
Reyer, E.
Ridley, G.
Riehlé Bros., Messrs.
Roberts, E.
Roberts, J.
Robinson, H.
Robinson, J.
Rodgers, Rear-Adm. J., U.S.N.
Rowan, F. J.
Rühlmann, Dr. M.

S.

Sadasewjee, H.
Sandberg, C. P.
Sandeman, J. W.
Sang, E.
Say, L.
Schneider & Co., Messrs.
Schreiber, C.
Schwebelé, E.
Scott, R. H.
Seddon, Maj. H. C.,
Selwyn, A. R. C.
Sennett, R.
Shaw, Capt. E. M.
Shaw, H. S. H.
Shock, W. H.
Shone, I.
Siemens, Dr. C. W.
Silver, S. W.
Smith, Prof. R. H.
Smith, R., Jun.
Smith, W. P.
Smythe, C. P.
Spice, R.

Spice, R. P.
Spratt, Vice-Admiral F. A. B.
Stanley, W. F.
Stayton, G. H.
Stevens & Haynes, Messrs.
Stirling, P.
Stone, T. W.
Street, W. C.
Sutter, A.
Swank, J. M.
Symon, A.
Symons, G. J.
Systermanns, A.

T.

Tarbotton, M. O.
Taylor, Capt. A. D., I.N.
Teague, W., Jun.
Thoms, G. E.
Thurston, Prof. R. H.
Tommaso, L.
Trélat, E.
Trowbridge, Prof. W. P.

U.

Unwin, Prof. W. C.

V.

Van Nostrand, D.
Vauthier, L. L.
Vernon-Harcourt, L. F.
Vetch, Maj. R. H., R.E.
Vétillart, M.
Vincent, C.

W.

Waring, G. E.
Waugh, J.
Welch, E. J. C.
Werther, G.
Wex, G. Ritter von
White, G.
White, T. W. R.
White, W. H.
Whitley, H. M.
Whitton, J.
Whitworth, Sir J., Bart.
Wicksteed, Rev. C.
Williams, J. S.
Wood, H.
Woolcott, C. H.
Wright, Major-Gen. H. G., U.S.A.
Wright, T. A.
Wright, W. B.

The Subscriptions to the Library Fund were as under:—

	£.	s.	d.
Addison, P. L.	1	1	0
Allen, P. R.	1	1	0
Allmann, C. X. J.	1	1	0
Anderson, G.	3	3	0
Anderson, J.	1	1	0
Anderson, R. S.	1	1	0
Barron, C.	2	2	0
Bean, A. W. T.	2	2	0
Betts, E. P.	2	2	0
Blandy, J. E. A. J.	1	1	0
Boys, W. J.	5	5	0
Brock, Walter	10	10	0
Brown, A.	1	1	0
Bulmer, P.	1	1	0
Cooper, C. H.	1	1	0
Corry, J. E.	0	7	9
Cox, J. H.	2	2	0
Cox, T. A.	1	1	0
Cubitt, W.	2	2	0
Duncan, Hon. G. A. H.	5	0	0
Eachus, G. E.	1	1	0
Eldridge, J., Jun.	1	1	0
Ellery, C. T.	1	1	0
Farewell, C. W. F.	1	11	6
Fforde, Jas.	5	5	0
Fiddes, W.	2	2	0
Foot, G.	20	0	0
Foote, A.	2	2	0
Fraser, E. G.	5	5	0
Fraser, J.	2	0	0
Garnett, G.	2	2	0
Gravell, D.	5	0	0
Grice, E. J.	5	5	0
Grimes, J.	2	2	0
Hanson, J. H.	2	2	0
Harpur, W.	1	1	0
Hodson, G.	2	12	6
Carried forward	106	18	9
Brought forward	106	18	9
Inglis, W.	2	2	0
Jackson, E. W.	10	0	0
Kempson, C.	1	1	0
Kennedy, Prof. A. B. W.	2	2	0
Kennedy, N.	2	2	0
Ker, C. B.	5	0	0
Lambert, G. N. R.	2	2	0
Larsen, J. D.	1	11	6
Linacre, J.	1	1	0
Lingard, J. E.	1	1	0
Maclachlan, A.	1	11	6
MacLennan, J.	1	1	0
Manwaring, H. H.	1	1	0
Martin, E. P.	10	10	0
Miller, L. M.	1	0	0
Moline, W.	1	1	0
Monckton, M. J.	1	1	0
Morris, T. B.	5	5	0
Muir, E.	1	1	0
Newton, F. M.	1	1	0
Nice, A. S.	1	1	0
Nicholson, W. S.	1	1	0
Parry, J. W.	1	1	0
Peacock, R.	21	0	0
Pendlebury, J.	1	1	0
Pope, F. T.	2	2	0
Primrose, A.	3	3	0
Read, R.	1	1	0
Reid, D.	5	5	0
Rhodes, W.	5	5	0
Salter, J.	1	1	0
Saubergue, A. de	1	1	0
Scobie, M. J.	5	5	0
Sheward, E. G.	1	1	0
Simon, F.	2	10	0
Smith, W.	5	0	0
Carried forward	217	11	9

	£.	s.	d.
Brought forward	217	11	9
Stent, S.	10	10	0
Stevenson, E. H. .	1	1	0
Strachan, John . .	2	2	0
Stuart, R. H. F. . .	2	2	0
Townsend, F. E. . .	1	0	0
Truman, A. S. . . .	3	3	0
Tysoe, J.	1	1	0
Carried forward	238	10	9

	£.	s.	d.
Brought forward	238	10	9
Western, M. R. . . .	2	2	0
Williams, C. St. M. .	1	1	0
Wingfield, C. H. . .	1	1	0
Woolcock, H.	1	1	0
Worth, J. E.	5	5	0
Worthington, W. B.	1	1	0
Young, B. H. . . .	2	2	0
	£252	3	9

OFFICERS.—1882-83.

Council:

PRESIDENT.

JAMES BRUNLEES, F.R.S.E.

VICE-PRESIDENTS.

Sir Joseph W. Bazalgette, C.B.,
Sir Frederick J. Bramwell, F.R.S.,
Edward Woods.
George Barclay Bruce.

MEMBERS.

Benjamin Baker,
John Wolfe Barry.
George Berkley,
Sir John Coode,
Edward Alfred Cowper,
Sir James Nicholas Douglass,
Alfred Giles,
Harrison Hayter,
William Pole, F.R.SS. L. & E.,
Robert Rawlinson, C.B.,
Alex. Meadows Rendel, M.A.,
C. William Siemens, F.R.S.,
David Stevenson, F.R.S.E.,
Sir Wm. Thomson, F.R.SS. L. & E.,
Sir Jos. Whitworth, Bt., F.R.S.

Honorary Councillors:

PAST PRESIDENTS.

Sir John Hawkshaw, F.R.S.,
John Fowler,
Charles Hutton Gregory, C.M.G.,
Thomas Hawksley, F.R.S.,
Thomas Elliot Harrison,
George Robert Stephenson,
John F. Bateman, F.R.SS. L. & E.,
William Henry Barlow, F.R.S.,
James Abernethy, F.R.S.E.
Sir W. G. Armstrong, C.B., LL.D., D.C.L., F.R.S.

Officers:

AUDITORS.

John Clarke Hawkshaw, M.A.
Henry Marc Brunel.

TREASURER.

Hugh Lindsay Antrobus.

HONORARY SECRETARY.

Charles Manby, F.R.S.

SECRETARY.

James Forrest.

⁂ *This Council will continue till December* 1883.

SECTION II.—OTHER SELECTED PAPERS.

(*Paper No. 1844.*)

"On the Adhesive Strength of Portland Cement with special Reference to an Improved Method of Testing that Material."

By Isaac John Mann.

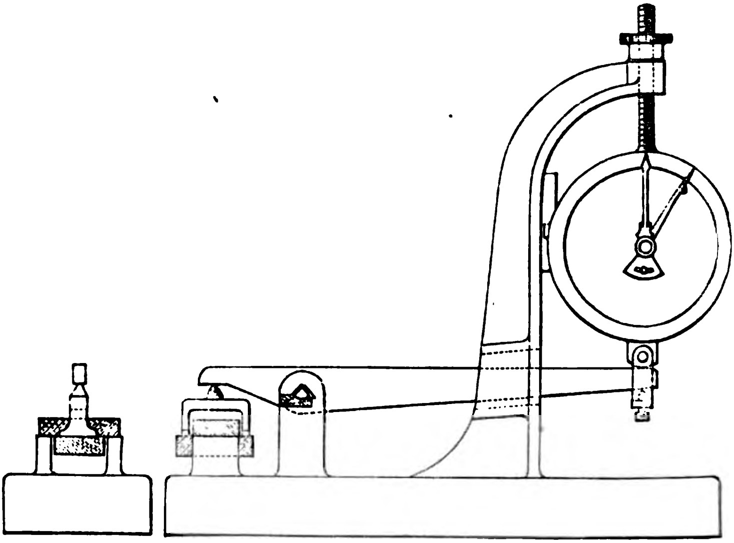
This communication refers principally to an investigation of the adhesive or cementitious strength of Portland cement, and a description of an improved method of determining its quality and value, with a view to establish a simple and generally recognised standard system of testing.

The Author believes that it may be fairly assumed that the principal function of cement is to produce adherence, so as to convert loose or disconnected material into a solid coherent form; and that it may be further assumed that the economy with which this object can be gained will, *cæteris paribus*, be in proportion to the adhesive strength of the cement employed.

Reviewing the history of cement-testing, and the experimental researches connected with it, the conclusion seems almost inevitable that neither of the systems at present in general use—one depending on the cohesive strength of neat cement, the other on that of a mixture of cement and sand—are likely to become universal.

The principal test adopted by the Author is one of adhesion. It has been used by him for some years with very satisfactory results, and he believes it will be found to possess elements which recommend it strongly for general adoption. The mode of applying the test is extremely simple, involving neither skill nor experience on the part of the operator, and most, if not all the complications of details incident to other systems can be avoided. The simple and inexpensive testing machine designed by the Author is shown in Fig. 1. It consists of a steel lever with a knife edge bearing on the fulcrum; the strain is applied by a thumb-screw, and registered by an accurate spring-balance, the dial of which is provided with a maximum index-hand; the samples are cruciform in shape, and consist each of two pieces of sawn limestone or ground plate-glass, 1½ inch by 1 inch, by ¼ inch to ⅜ inch. When a sample is to be broken, it is placed on two vertical supports under the end of the short arm of the lever. An adapter, with a steel centre-point fitting into a small conical recess at the end of the lever, enables the strain to be brought on the sample with great accuracy and facility, and a cushion of soft wood or india-rubber fixed in the adapter checks the recoil after fracture. The centre-point can be provided with a screwed end, to permit adjustment in case the cement joint should inadvertently be made thicker than necessary; this, however, is an unlikely contingency. Limestone or ground plate-glass are suitable for standard tests; the former because it is readily obtainable, of sufficiently uniform texture, and enters largely into construction; the latter on account of its being homogeneous and easy to obtain.

Fig. 1.



Cement-Testing Machine.
Scale ¼th real size.

The cement to be tested is gauged to a suitable consistency, and applied with a spatula to one of the test-pieces; the other being placed in position, a slight pressure is sufficient to squeeze out the superfluous cement, which forms a fillet tending to protect the extreme edges of the joint from any "wash" that might occur in the act of immersing the sample, or at any time previous to setting, and which can be removed after the cement has set. When the samples are made they are numbered, and at once placed in water in shallow vessels, which are also numbered, or lettered, to facilitate identification. After fracture the remaining cement is removed from the test-pieces, which can be used as often as required.

The transition from the German or mortar test to one of adhesion is not so abrupt as would appear: in the former a great number of small pieces of stone are used, in the form of sand; in the latter two pieces only are employed, and the test is thus simplified and better defined. The German test, however, is neither one of cohesion only nor of adhesion, but involves an indeterminate proportion of each, which cannot be considered desirable.

In his investigations of the cementitious or adhesive strength of cement, the Author was necessarily obliged to devote considerable attention to the influence of the coarse particles. The test of adhesion alone, even without any qualification as to grinding, would be almost sufficient to determine the practical value of cement, the weakening effect of the coarse particles becoming at once apparent, as will be seen from the following tests:—

TABLE I.—EFFECT of COARSE PARTICLES on the CEMENTITIOUS or ADHESIVE STRENGTH. Age of samples, twenty-eight days.

Percentage of coarse particles	0 fine only	20	40	80	100 coarse only
Average adhesive strength in lbs. per square inch . .	101	84	57	34	18

Percentage of coarse in the unsifted cement, 49. Cohesive strength of the same after seven days, 430 lbs. per square inch. Number of tests, twenty. Test-pieces, sawn limestone.

The coarse particles referred to above were those stopped by a silk sieve of 176 meshes to the lineal inch, the meshes being about 0·004 inch by 0·004 inch; a sieve of such fineness, although not heretofore used in ordinary testing, has been found to afford more definite and reliable results than those having larger meshes.

Another series of tests gave the following results after seven days' immersion; the same sieve being used to produce the required degree of coarseness:—

Fine cement only	91	lbs. per square inch.
25 per cent. of coarse particles . .	63	,, ,,
75 ,, ,, . .	26	,, ,,
Coarse particles only	8	,, ,,

Cohesive strength of the cement as supplied, 532 lbs. per square inch. Age, seven days.

Although these, and numerous other tests of a similar character, have been sufficiently satisfactory and conclusive, the presence of coarse particles may be regarded as introducing an element which might possibly help to interfere with perfect uniformity in results, and which might therefore with advantage be eliminated in a standard test; a simple and obvious alternative will be found sufficient to overcome the objection referred to.

It can hardly be doubted that the strength of a joint made with ordinary cement must be influenced by the fortuitous position which the coarse particles occupy; or, in other words, by the proportion of inert, or comparatively inert particles which happen to be in direct contact with the cemented surfaces. In accordance with this view, the Author proposes that one of the standard adhesive tests should be applied after the inert particles have been removed by sifting through a standard sieve.

Cement, as at present received from the manufacturer, so far as concerns the cementitious strength capable of being developed in the period to which ordinary testing must of necessity be limited in practice, consists of a mixture of active, and inert, or extremely sluggish material, and the latter may be considered almost as foreign to the true cementitious portion, as so much sand.

This view is supported by the following experiment: The coarse particles stopped by a No. 176 sieve, amounting to 49 per cent. of the cement as supplied, were removed, and sand of approximately the same granulation substituted; the average adhesive strength of this compound or mortar, compared with that of the ordinary cement after seven days' immersion, was—

Sand and fine cement	49 lbs. per square inch.
Ordinary cement	56 ,, ,,

Test-pieces, sawn limestone.

Separation of the inert and active portions would be manifestly desirable, as forming, in combination with an adhesive test of the latter, a simple means of arriving at a true estimate of both the cementitious and commercial value of any given cement, in much the same way as the value of an ore is estimated by the percentage of metal it contains. The Author's attention was therefore directed to this part of the subject, with a view to discover, if possible, the degree of pulverisation required to convert the unground clinker into active cement, capable of developing its cementitious properties within the period allowed in practice for testing purposes.

Some of the principal results of the experiments, up to the present time, are contained in the following Table; unfortunately others applying to lengthened periods had to be discarded, owing to accidental exposure to frost, which in many cases seemed almost to destroy the adhesive strength. In several instances the coarse particles were rapidly washed in two or three waters, to remove a minute quantity of fine cement which adhered to them after sifting. The No. 176 sieve, used to separate the coarse particles, was the finest that could be obtained, arresting from 38 to 50 per cent. of the ordinary cement. No. 103 sieve retained from 25 to 30 per cent.; the unsifted cements required from forty minutes to five hours to set in air, the temperature of which varied from 50° to 70° Fahrenheit. They were obtained from well-known manufacturers, and had an average cohesive strength, after seven days, of 425 lbs. per square inch, their average adhesive strength being 61 lbs. per square inch, and 84 lbs. per square inch after seven and twenty-eight days' immersion respectively.

TABLE II.—ADHESIVE STRENGTH of the COARSE PARTICLES of PORTLAND CEMENT.

Age.	Average Strength in lbs. per Square Inch.	Degree of Pulverisation.
7 days	1	Stopped by No. 176 sieve and washed.
7 ,,	0	54 per cent. of fine particles removed by washing only.
7 ,,	6	Fine removed by No. 176 sieve; very coarse by No. 54 sieve.
8 ,,	8	Particles which passed No. 176 sieve, after very fine had been removed by ten minutes' sifting.
8 ,,	0	Stopped by No. 176 sieve and washed.
8 ,,	6	Fine removed by No. 158 sieve; very coarse by No. 54 sieve.
9 ,,	0	Stopped by No. 103 sieve.
9 ,,	½	Stopped by No. 176 sieve and washed.
28 ,,	12	Particles which passed No. 176 sieve, after 46 per cent. of fine had been sifted out.
28 ,,	14	Fine removed by No. 176 sieve; very coarse by No. 54 sieve.
28 ,,	5	Stopped by No. 176 sieve.
28 ,,	18	,, ,,
28 ,,	20	Fine removed by No. 176 sieve; coarse by No. 103 sieve.
28 ,,	1	,, No. 103 sieve; very coarse by No. 54 sieve, and washed.
28 ,,	9	Stopped by No. 176 sieve, and washed.
28 ,,	14	Fine removed by No. 158 sieve; very coarse by No. 54 sieve.
28 ,,	18	Fine removed by No. 176 sieve; very coarse by No. 54 sieve, and washed.
28 ,,	0	Fine removed by No. 176 sieve; coarse by No. 103 sieve.
28 ,,	2	,, ,,
28 ,,	3½	Stopped by No. 176 sieve, and washed.
28 ,,	24	Passed No. 176 sieve, after very fine had been removed by sifting for two minutes.
15 weeks	20	Fine removed by No. 176 sieve; very coarse by No. 54 sieve.
15 ,,	14	Stopped by No. 176 sieve and washed.

Number of tests, eighty. Test-pieces, sawn limestone.

A set of samples eleven months old, made from cement passing a No. 54 sieve and stopped by a No. 103 sieve, but not washed, and having therefore a minute quantity of very fine cement adhering, showed an average adhesive strength of 21 lbs. per square inch.

The figures given in the Table are in some cases the average of six tests, but in general that of three. In these, as in all the other experiments on the adhesive strength recorded in this Paper, the test-pieces, unless otherwise stated, were of sawn close-grained limestone, and were placed in fresh water immediately after being cemented together.

The above examples, although far from exhaustive, lead to the following conclusions:—

1. That so far as concerns a seven-days' test, the particles of cement stopped by a No. 176 sieve developed little or no cementing power during that period, and that even some of the less fine particles passing the sieve may be very deficient in cementitious strength. This was further shown to be the case by the following tests: A weight of about 300 or 400 grains of ordinary cement was placed in the sieve (No. 176); after sifting gently for thirty seconds, a quantity of the very finest passed through, which was made into samples, the cement passing through the sieve during the following two minutes was rejected, the sifting being continued until sufficient cement to make four samples had passed, leaving, however, a large portion still in the sieve. The adhesive strength of the former samples was much greater than that of the latter when tested after seven days' immersion. This was repeated, using the cement of other manufacturers, with the same result.

2. That so far as concerns a twenty-eight days' test, the cement of different manufacturers varied in the cementitious strength of the particles stopped by No. 176 sieve, from nothing to 20 lbs. *per* square inch, their strength increasing but slowly in the longer periods, and probably becoming soon exhausted.

No. 176 and No. 103 sieves were of silk, the meshes being respectively about 0·0040 inch and 0·0075 inch square, and slightly smaller than those of the same numbers formed of wire. The Author has since, through the courtesy of Dr. Michaëlis, of Berlin, been furnished with a woven brass wire sieve, of almost the same fineness as No. 176 silk sieve. The time required to sift 400 grains weight, using a sieve about 4 inches in diameter, varied from fifteen to twenty minutes, the operation being facilitated by the addition of a few small round pebbles.

If any general conclusion can be drawn from the Table, which represents only eighty tests, it would appear to be that the cementing energy of coarse particles develops much more slowly than that of the fine particles. The necessity of adopting a high standard of pulverisation is also shown. For example, the particles which were sufficiently fine to pass a sieve of ten thousand six hundred meshes to the square inch, viz. No. 103, possessed less than one-fifth of the cementitious value of those passing a sieve of thirty-one thousand meshes per square inch, viz. No. 176.

The adoption of a high standard of pulverisation need cause no apprehension to manufacturers, as the percentage of cement stopped by a standard sieve, and the price, can be mutually accommodated to meet all the commercial exigencies of the case.

The degree of pulverisation of the cement supplied by some of the principal manufacturers fluctuates between comparatively narrow limits, as may be seen from the following Table :—

TABLE III.

Maker.	Percentage by Weight stopped by a No. 176 Sieve.	Maker.	Percentage by Weight stopped by a No. 176 Sieve.
F	49	L	49
D	47	S (quick setting)	41
G	45		
G	46	S (slow setting)	47
H	43	Y	38
I	47		
K	50	General average	45·6

A slight modification only is required to adapt ordinary specifications of cement to the terms proposed by the Author.

The next point requiring investigation is the adhesive or cementitious strength of the fine particles. For this purpose the finest sieve obtainable, viz. No. 176, was used, and the cement sifted until an inappreciable amount escaped through the meshes.

In the two following Tables will be found some of the principal results obtained from cements of ten different manufacturers, requiring from forty minutes to five hours to set in air. Extremely quick- and very slow-setting cements were reserved for Table XII.

TABLE IV.—ADHESIVE STRENGTH of FINE CEMENT SIFTED THROUGH No. 176 SIEVE. Age, seven days. Test-pieces, sawn limestone.

Manufacturer . . .	A	B	B	C	D	D	E	B	C
Cementitious strength in lbs. per square inch	61	101	101	69	73	91	85	100	84

Manufacturer . . .	A	F	G	A	H	I	K	F	G
Cementitious strength in lbs. per square inch	70	57	65	74	63	83	66	81	82

Number of tests, sixty-two. General average, 78 lbs. per square inch.

TABLE V.—ADHESIVE STRENGTH of FINE CEMENT SIFTED THROUGH No. 176 SIEVE. Age, twenty-eight days. Test-pieces, sawn limestone.

Manufacturer	H	C	D	I	B	H	F	L	L	K
Cementitious strength in lbs. per square inch . .	71	121	66	84	110	100	77	88	109	105

Number of tests, thirty-eight. General average, 93 lbs. per square inch.

Considerable differences are apparent, not only in the cements of different manufacturers, but also in different cargoes from the same maker, and occasionally in the same cargo. Most of the tests relating to longer periods were unreliable from exposure of the samples to frost; a limited number, which escaped exposure to frost, had an average adhesive strength of 116 lbs. per square inch after thirteen weeks' immersion, while others of a different manufacturer, which had been in water for six months, had a strength of 113 lbs. per square inch. A set of samples, which had been some months immersed before being frozen, and therefore better able to resist injury from this cause, had an average cementitious strength, after fifteen months, of 173 lbs. per square inch, one of the samples exhibiting the highest development of adhesive strength which the Author has yet observed, namely, 240 lbs. per square inch. The weight of fine cement from which this cementitious energy was developed did not exceed 5 grains. This strength is equivalent to nearly 16 tons per square foot; but few, if any, masonry-joints are ever subjected to a tearing strain of this severity.

Owing to the Author preferring to make all the tests himself, and to the limited time at his disposal, the averages represented by each of the figures in the Tables have been derived from three to six tests. In ordinary testing, however, it would be desirable to take the averages from not less than six tests.

Although the test proposed to be applied, after removing the inert particles by an extremely fine sieve, will probably give the most trustworthy and uniform results, the adhesive test should be also applied to the cement as received from the contractor, and as actually used in construction, this test alone being almost sufficient to enable a correct estimate to be formed of the value of the cement.

In the following tests of the cementitious strength of ordinary cement the extremes of quick and slow setting were avoided. In every case, unless otherwise stated, the cement was immersed in fresh water immediately after the test-pieces were joined:—

TABLE VI.—ADHESIVE STRENGTH of ORDINARY CEMENT as RECEIVED from the MANUFACTURER. Age, seven days. Test-pieces, sawn limestone.

Manufacturer . .	G	K	I	F	F_1	G_1	G_2	H
Cementitious strength in lbs. per square inch	76	57	51	69	71	73	59	56
Manufacturer . .	**G_3**	**W**	**F_2**	**W_1**	**W_2**	**G**	**F_2**	**K_2**
Cementitious strength in lbs. per square inch	52	50	54	59	41	37	51	56

Number of tests, sixty. General average, 57 lbs. per square inch.

The cements of well-known makers were used, and with one or two exceptions the results are tolerably uniform, considering that nearly 50 per cent. of the material tested was practically inert, and its position relative to the surfaces of the test-pieces uncertain. The highest and lowest figures in the Table were obtained from the same cement; it was however the slowest in setting, and nearly twelve months elapsed between the two tests. The test-pieces should in every case be scrupulously clean, and allowed to remain in water for a short time before using.

Table VII. shows some of the principal results obtained from a twenty-eight-day test of ordinary cement.

TABLE VII.—ADHESIVE STRENGTH of ORDINARY CEMENT as RECEIVED from the MANUFACTURER. Age, twenty-eight days. Test-pieces, sawn limestone.

Manufacturer	H	H_2	K	K_1	F	F_1	W	W_2	G	G_1
Cementitious strength in lbs. per square inch	78	57	48	71	98	69	91	84	108	75

Number of tests, thirty-six. General average, 78 lbs. per square inch.

Estimated by their cohesive strength the cements were all of good quality. The differences are not so great as those in the preceding Table, and no doubt can be partly accounted for in the same way. Of course the cements of different makers could not be expected to show perfectly uniform results, but the averages would probably be only slightly altered if obtained from a larger number of tests.

Table VIII. shows the results of a limited number of tests after thirteen weeks' immersion of the samples.

TABLE VIII.—ADHESIVE STRENGTH of ORDINARY CEMENT, as RECEIVED from the MANUFACTURER. Age of samples, thirteen weeks.

Average Strength in lbs. per Square Inch.	Remarks.
110	Test-pieces, sawn limestone. Samples made with and immersed in sea-water.
113	Test-pieces, ground plate glass. Fresh water.
60	Test-pieces, sawn limestone. Fresh water.
110	Test-pieces, sawn limestone. Fresh water.
Average 98 lbs. per square inch.	

A series of samples which had been immersed for seven months had an average strength of 128 lbs. per square inch. Another set which had been frozen, had a strength after twelve months of only 40 lbs. per square inch, while some samples of the same cement, but with the coarse particles removed by a No. 176 sieve, although similarly frozen and of the same age, had an average strength of 107 lbs. per square inch. The comparative cementitious strength of the sifted and unsifted cements tested by the Author is given in Table IX.:—

TABLE IX.—COMPARATIVE CEMENTITIOUS STRENGTH of SIFTED and UNSIFTED CEMENT.

Description.	Averages in lbs. per Square Inch.		
	Age, Seven Days.	Age, Twenty-eight Days.	Age, Thirteen Weeks.
Cement with the coarse particles removed by No. 176 sieve	78	93	116
Ordinary cement as received from the manufacturer	57	78	98

These results are satisfactory, particularly with reference to their bearing on the subject of the deposition of concrete, or rubble-in-concrete, in deep water, a method of construction which can be applied with such economy and rapidity of execution as to supersede the slow and costly methods which involve the use of cofferdams or large blocks. The adhesive strength shown by the Author's experiments indicates the great stability and monolithic character of structures in which Portland cement is employed as the cementing material, and immersed before setting. In several instances when the test-pieces consisted of comparatively soft but otherwise sound stone, such as Portland and sandstone, the adhesive strength of the cement was sufficient to tear small fragments out of the surfaces of the test-pieces, the age of the samples being only twenty-eight days, and the breaking-strain slightly below the average of ordinary unsifted cement (Table XV.).

In drawing any inferences from Table IX. it should not be forgotten that to produce the strength shown in the second line of figures involved the use of, at least, four times the quantity of cementing material required to produce the superior strength shown by the figures in the first line, or those referring to the sifted cement, owing to the difference in the thickness of joint caused by the presence of the coarser particles. The averages given above are derived from tests applied at various times to the cements of twelve different makers. An abstract of some of the principal tests applied to the same cement, or that made by one of the manufacturers, is given in Table X.:—

Table X.—Abstracts of principal Tests applied to the same Cement.
Test-pieces, sawn limestone.

Age.	Average Adhesive Strength in lbs. per Square Inch.	Remarks.
1 day .	15	Ordinary cement.
3 days .	36	„ „
7 „	76	„ „
7 „	73	„ „ 2nd set of samples.
7 „	91	Fine only, separated with No. 176 sieve.
7 „	63	„ with 25 per cent. of coarse added.
7 „	26	„ „ 75 „ „
7 „	8	The coarsest particles that passed through No. 176 sieve.
7 „	0	Stopped by No. 176 sieve.
7 „	1	„ „ another set of samples.
28 „	88	Ordinary cement.
28 „	84	„ gauged with and immersed in sea-water.
28 „	120	„ „ exposed to air and sea-water each alternate day.
15 weeks	20	Fine, removed by No. 176 sieve, very coarse by No. 54 sieve.
15 „	14	Stopped by No. 176 sieve and washed.

Average strength of cohesion of the ordinary cement as supplied after seven days, 480 lbs. per square inch. Pulverisation, 45 per cent. of the ordinary cement stopped by No. 176 sieve.

The relation, if any, between the cohesive and adhesive, or cementitious, strength of neat cement seems to be extremely obscure, as might be anticipated from the fact that the presence of coarse particles within certain limits increases the former but diminishes the latter. The following examples will serve to show that the ordinary seven-days' test of tensile or cohesive strength is unreliable as an exponent of the cementitious value.

TABLE XI.—COMPARISON of ADHESIVE and COHESIVE STRENGTH.

No.	Description.	Average strength in lbs. per Square Inch.	
		Adhesive.	Cohesive.
1	Ordinary cement, age seven days	59	532
2	" "	51	336
3	Fine cement sifted through No. 176 sieve, age seven days.	94	428
4	" " " "	57	345
5	" " " "	65	500
6	" " " age twenty-eight days	105	500
7	" " " "	109	387
8	" " " "	84	428
9	" " " "	110	309
10	" " " "	85	320

The Author believes that the cohesive test has not unfrequently led to the rejection or condemnation of excellent cement. For example, in experiment No. 9, Table XI., the low tensile strength of the cement would probably have caused its rejection, while its cementitious strength was considerably above the average of good cements.

The next point requiring investigation is the effect of quick- and slow-setting on the adhesive strength, which is capable of being developed within the limits of the time available for ordinary testing purposes. Whether the time occupied in setting affects the ultimate strength of adhesion or otherwise must be reserved for future investigation.

Table XII. contains some of the Author's experiments relative to this part of the subject (p. 15).

It will be noticed that, with one or two exceptions, the quick-setting cement manifested a greater development of adhesive strength than the slow, while in the case of cohesive strength quick-setting seems generally to produce an opposite effect. In experiments 1, 3, 5, 7, and 8, slow setting was produced by air-slaking. In Nos. 2, 4, and 6, the cements used were sent by

TABLE XII.—ADHESIVE STRENGTH of QUICK- and SLOW-SETTING CEMENT. Test-pieces, sawn limestone.

No.	Degree of Pulverisation.	Age.	Time Setting in Air.	Average Strength in lbs. per Square Inch.	Remarks.
1	ordinary	7 days .	40 mins.	57	As received from makers.
"	"	7 " .	8 hours	29	Cooled by air-slaking.
2	fine . .	7 " .	10 mins.	58	As received from makers.
"	" . .	7 " .	5 hours	85	" "
3	" . .	7 " .	40 mins.	101	" "
"	" . .	7 " .	3 hours	41	Cooled by exposure to air.
4	ordinary	7 " .	1½ "	46	As received from makers.
"	"	7 " .	3¼ "	33	" "
5	"	28 " .	40 mins.	71	" "
"	"	28 " .	24 hours	48	Cooled by exposure to air.
6	fine . .	28 " .	10 mins.	67	As received from makers.
"	" . .	28 " .	5 hours	121	" "
7	" . .	13 weeks	30 mins.	110	" "
"	" . .	13 "	14 hours	71	Cooled by exposure to air.
8	" . .	6 months	30 mins.	113	As received from makers.
"	" . .	6 "	14 hours	91	Cooled by exposure to air.

Note.—"Fine" cement was that which passed No. 176 sieve.

different manufacturers as samples of quick- and slow-setting cement, and were not exposed to atmospheric influence.

The time of setting was arrived at by the following means: A vertical steel needle moving freely in guides, and having a flat point $\frac{1}{18}$ inch in diameter, was loaded so as to weigh 1 lb. When the pressure of the point made no visible mark on the surface of the gauged cement it was considered to be set. This is approximately the same pressure as that of the finger-nail, but it has the advantage of being more definite and reliable. Nos. 7 and 8 were samples of the same cement, and tend to show that, although the cementitious strength of the slow-setting samples developed slowly in the shorter interval, it subsequently appeared to gain on that of the quick-setting. In the examples referred to, the quick-setting gained only 3 lbs. in the second three months, while the slow-setting increased 20 lbs. in the same period. Experiment No. 2 was an extreme case, the cement showing incipient signs of hardening before the gauging was completed. The proportion of water used in gauging was limited to that required in each case to bring the cement to the same consistency.

The effect of very rapid setting is apparent in the case of orchard or quick-setting Medina, some samples of this cement showing an average adhesive strength of only 10 lbs. per square inch after seven days' immersion. Similarly, plaster of Paris had a strength of 12 lbs. per square inch after twenty-eight days in air, and Keene's cement 18 lbs. per square inch after seven weeks in air.

As regards a seven or twenty-eight-days' standard test, quick-setting Portland cement can be dealt with either by providing that it shall bear a somewhat higher strain, or by bringing the time in setting within defined limits by air-slaking; the former might possibly lead to complication, the latter is apparently more likely to meet the requirements of the case. The probability is, however, that if a carefully fixed standard of cementitious strength were generally adopted, neither of the alternatives mentioned would have to be resorted to, manufacturers finding it to be their interest to produce cement of sufficient uniformity as regards setting, and fit for immediate use.

In connection with this part of the subject, the proportion of water used in gauging requires consideration. As in the Author's system of testing so small a quantity of cement is gauged, he at first used a glass dropping-tube, furnished with a small flexible tube and pinch-cock, the weight of the drops being previously ascertained; the weight of cement to be gauged was generally 200 grains. However, the proportion of water that would enable the cement to be spread easily and uniformly on the test-pieces varied slightly with almost every sample, and the fine-sifted cement required more water than the same cement unsifted. The weighing of the water was therefore abandoned as introducing complications and refinements unsuitable to the practical requirements of a standard test. In all the tests recorded in this Paper (unless otherwise specified) the cements were gauged to approximately the same degree of consistency, an operation which involves but little skill or experience. The test-pieces were also immersed in water previous to making the samples, and the cement was applied while the surfaces were wet.

It was noticeable, in the case of the longer testing-periods, that after fracture the cemented surfaces of the test-pieces, and the thin wafer of cement between them, were quite dry; and in the case of wrought iron the surfaces were clean and bright, exhibiting no signs of oxidation; these and other considerations would lead to the conclusion that after taking the necessary quantity of water into combination, the cement resists all further absorption.

The length of time of setting in air is complicated by the effect produced upon it by heat and humidity; four samples of the same cement were gauged with the same proportion of water, two of them being placed in rather warm, dry air, the others in cool and somewhat damp air, when the former set in twenty-seven minutes, the latter in four hours, the needle test being used in each case.

Such interferences are avoided in the Author's system, by immersing the test-pieces immediately after the samples are made.

There appears to be a considerable difference in the hardening produced in air and water in the same time; for example, one cement of good quality was gauged in the usual manner, part being replaced in air and part in water; the former set in three hours. On examining the samples eighteen hours after gauging, the testing-needle, when loaded with 2¼ lbs., produced a visible mark on the sample immersed in water, but required to be loaded with 14¼ lbs. to produce a similar impression on the sample which had remained in air. As hydraulicity is the most important feature in Portland cement, if it should become necessary to take the time of setting into account, it would perhaps be more consistent and useful to note that time with respect to water rather than air.

Table XIII. contains some of the Author's observations of the relative time occupied by cement in setting in air and water, ascertained by the test-needle before referred to.

TABLE XIII.—RELATIVE TIME OCCUPIED BY CEMENT IN SETTING IN AIR AND FRESH WATER.

No.	Temperature. Fahrenheit.	Time required to Set in Air.	Time required to Set in Water.	Remarks.
	°	H. M.	H. M.	
1	41	1 35	3 30	Ordinary cement.
2	41	3 15	4 30	„ „
3	50	0 35	2 30	„ „ gauged very dry.
4	50	1 40	2 45	„ „ „ very wet.
5	50	0 23	0 55	Sifted through No. 176 sieve.
6	53	0 30	1 15	„ „ gauged rather dry.
7	54	0 9	0 20	Special quick-setting.
8	53	5 0	22 0	„ slow- „
9	55	0 20	2 0	Ordinary cement.
10	34	3 0	16 0	„ „
Average . . .		1 38	5 34	

No. 5 similar cement to Nos. 3 and 4, but sifted.
No. 6 similar cement to No. 2, but sifted.

Nos. 1 and 7 were sent by the manufacturers as samples of their quick-setting cement, Nos. 2 and 8 of their slow-setting; Nos. 3 and 4 were from the same cement, but gauged with a minimum and maximum amount of water respectively.

The only inferences that can be drawn from these experiments appear to be that fine-sifted cement sets faster, both in air and water, than ordinary cement, and that no definite relation exists between the respective times required to set in air and water. To ensure accuracy in the above results involved much time and trouble, and it would be advisable to eliminate such observations from any universally adopted tests.

The influence of water in gauging, on the cementitious strength, is somewhat capricious; an excess of water frequently produces an increase of strength compared with cement gauged very dry. Sometimes, however, the samples possessed the same strength in both cases, the samples being, in every instance, immersed as soon as they were made.

The strength of adhesion of Portland cement to different substances varied considerably; the roughness or smoothness of the cemented surfaces, however, did not affect the strength as much as had been supposed.

Table XIV. contains most of the results obtained by the Author relative to this part of the subject.

TABLE XIV.—STRENGTH of ADHESION of PORTLAND CEMENT to VARIOUS MATERIAL.

Material.	Average Adhesive Strength.				Remarks.
	7 Days.	28 Days.	13 Weeks.	6 Months.	
Bridgewater brick.	19	..	..	..	Ordinary cement.
„ „	24	66	..	..	Sifted through No. 176 sieve.
Slate (sawn) . .	49	..	..	..	Ordinary cement.
„ „ . .	53	82	..	62	Sifted through No. 176 sieve.
Portland stone. .	26	50	..	..	Ordinary. Fragments torn out of surface.
„ „ . .	29	62	..	55	Sifted through No. 176 sieve. Fragments torn out of surface.
Ground plate glass	..	102	113	..	Ordinary cement.
„ „	..	..	145	..	Sifted through No. 176 sieve.
Plate iron . . .	23	68	..	..	Ordinary.
„ . . .	44	66	..	..	Sifted through No. 176 sieve.
Sandstone . . .	..	49	..	..	Ordinary. Fragments torn out of surface.
Polished marble .	38	..	..	..	Ordinary cement.
„ „	52	71	..	75	Sifted through No. 176 sieve.
„ plate glass	47	40	70	..	Ordinary cement.
„ „	55	49	51	..	Sifted through No. 176 sieve.
Granite (chiselled)	41	..	..	..	Ordinary.
„ „	78	97	153	..	Sifted through No. 176 sieve.
Limestone, sawn .	57	78	98	..	Ordinary cement.
„ „	78	93	116	..	Sifted through No. 176 sieve.

Total number of tests (omitting those of sawn limestone), one hundred and eighty-two.

The best Portland cement, obtained from five leading manufacturers, was used in the course of these experiments.

The cement adhered to very hard surfaces, such as granite and ground plate glass, with much greater strength than to softer material, as Bridgewater brick, Portland, and limestone.

The strength of adhesion to polished surfaces was also remarkable; in the case of polished plate glass, the average adhesive strength of fine cement in one set of samples was, after twelve months' immersion, 125 lbs. per square inch; the surfaces of the thin wafer of sifted cement joining the test-pieces, particularly in the longer periods, possessing the same fine polish as the glass, so much so as to suggest the use of polished cement for ornamental purposes. Some experiments made with oak, both in air and water, showed so small an amount of adhesion as to be hardly appreciable.

In comparing the results obtained from ordinary cement and that sifted through No. 176 sieve, the relative quantities of the cementing material, and other considerations referred to, should be borne in mind. Some experiments were made in which the cement was gauged with, and immersed in, sea-water, to compare with others made at the same time, in which fresh water was used. The results were as often favourable to one as the other, the averages being almost identical; the number of tests, however, was but forty, and the time from one week to five weeks. A few tests were also made to try the effect, on ordinary cement, of breaking the bond; the results showed an average strength of 8 lbs. and 32 lbs. per square inch, after seven days and thirteen weeks respectively, the bond being broken (*i.e.* the test-pieces disconnected and then replaced in position) twenty-four hours after the samples were made, but the limited number of experiments considerably diminish the value of these results.

The most suitable shape for the test-pieces had to be determined by experience, involving repeated trials; the first shape suggested was that ultimately adopted, namely, cruciform. It was, however, discarded at first, owing to the want of a suitable testing-machine. In the preliminary experiments a slab of stone was introduced into the neck of the ordinary cohesive sample, an arrangement which, unknown to the Author, had been adopted by Dr. Michaëlis about the same time. This was found cumbersome and tedious; it was also difficult to ensure perfect contact between the stone and cement, and it almost precluded any lengthened series of tests with the fine and coarse particles of cement, owing to the time required to sift so large a quantity as would be necessary. Cubes of stone, with holes drilled in the sides to receive suitable clips were next tried, but they were also cumbersome and expensive, and the results likely to be unsatisfactory. The majority of the experiments referred to in this Paper were made with test-pieces, one of which was 2 inches by 2 inches by $\frac{3}{8}$ inch, having a hole rather less than $\frac{3}{8}$ inch in diameter drilled through the centre, the other being $1\frac{1}{16}$ inch by $1\frac{1}{16}$ inch by $\frac{3}{8}$ inch. So that, deducting the area of the hole, the cemented surfaces had an area of 1 square inch. The cement was placed on the smaller piece, which was then fixed centrally on the larger, and a slight pressure used to ensure perfect contact; a steel pin, fitting the hole loosely, afforded the means of applying the force necessary to separate the test-pieces. This arrangement works satisfactorily, but involves a little trouble in removing the superfluous cement pressed out from between the test-pieces into the hole in the under one, and which, if not removed before testing, might possibly cause the end of the steel pin to be wedged.

To render such a contingency impossible, the cruciform shape was adopted, and the Author believes it will be found to meet all the requirements for accurate testing. The testing-machine already described permits the use of either drilled or cruciform test-pieces. The neatness, accuracy, and facility with which tests can be made by the Author's method, he ventures to think, will be appreciated by those who have had experience in testing cement by its cohesive strength, either neat or mixed with sand.

The advantages of a universal test, recognised alike by manufacturers and consumers, can hardly be overrated; at present manufacturers are considerably perplexed and inconvenienced by the varying conditions imposed by engineers, and others, but would be willing to work to a carefully arranged practical standard if such were agreed upon.

The total number of adhesive tests made by the Author has exceeded one thousand two hundred, a number which, however, must be considered somewhat limited when the nature, properties, and uses of the material are taken into account.

In adopting the adhesive test, the usual specification of the quality of English Portland cement requires to be modified to the following effect:—

"The cement shall be ground so that not more than [45] per cent. shall be stopped by a No. [176] silk sieve, and its average adhesive strength after twenty-eight days' immersion shall be as follows:—

Cement passing No. 176 sieve not less than [95] lbs. per square inch.
Cement as supplied for use " " " [75] " " "

Six tests being employed in each case."

If desired a seven days' test can also be specified. Such a modification will involve but little extra trouble to manufacturers, some of whom now produce cement of considerably finer pulverisation than that above indicated.

With reference to a standard test, the Author's investigations and remarks may be briefly summarised as follows:

That the cementitious or true value of Portland cement can be best determined by testing its adhesive strength.

That the degree of pulverisation is probably the only other condition, the practical importance of which will warrant an introduction into a standard system, which should therefore include a standard sieve. That a sieve having one hundred and seventy-six meshes to the lineal inch will be found sufficient for all practical purposes.

That so far as regards the limited time which can be allowed in practice for testing before use, an estimate of the quality of cement can be best obtained by testing the cementitious strength of the fine particles capable of developing most of their strength within the period referred to, but that this should not prevent an adhesive test from being applied to the cement in its ordinary condition.

That any complications arising from differences in the adhesive strength produced by quick- and slow-setting should, if possible, be eliminated from a standard system of testing. With respect to the time to be allowed for testing, the Author believes that the recognised periods of seven and twenty-eight days can be made sufficient and should be retained, longer periods being desirable, but precluded by the practical inconvenience involved. If the seven-days' test was satisfactory, and necessity should arise, the cement could be used without further delay; but, as a rule, judgment should be reserved until after the result of the twenty-eight-days' test was known. As regards what has been called the "weight-test," the Author believes it might with advantage be omitted, but if desired, the density could be ascertained in the manner described by the Author in a former Paper.[1]

---

[1] Minutes of Proceedings Inst. C.E., vol. xlvii., p. 251.

(*Paper No. 1864.*)

"The Capacity of Storage-Reservoirs for Water-Supply."

By W. Rippl, Docent at the Royal Technical High School
at Gratz (Styria).

1. Introduction.

In the English system for the water-supply of towns, by collecting the drainage of large catchment-basins, one of the most important problems is the determination of the capacity for storage, which should be provided in the reservoirs.

In the earlier works designed on this plan this point did not receive sufficient attention, because at that time the data required were not available. Hence reservoirs were constructed of insufficient size, causing a sensible deficiency in the water-supply in dry seasons. As the dams of the storage-reservoirs could not be raised in height without endangering their stability, new reservoirs and new gathering-grounds had to be added—a proceeding sometimes difficult and always costly.

For a long time engineers were obliged to apply the results of experience gained in existing waterworks to the design of new systems, by giving to the reservoirs a fixed capacity for a given area of gathering ground. If, for example, in an existing system of water-supply, a storage-capacity of 2,500 cubic metres (88,288 cubic feet) was found adequate for 1,000 hectares (2,471 acres) of gathering-ground, the reservoir of a new system was designed to afford a proportionate storage-capacity. But as the amount of storage necessary depends on circumstances which vary in different localities, it is clear that in reservoirs thus designed, it is only by accident that a deficiency of water-supply, in a series of years, is prevented.

2. The ordinary Formula.

The purpose of the storage-reservoir is to equalise the fluctuations of supply and demand during an indefinitely long period of time. The circumstances of an average year are therefore not sufficient to determine the quantity to be stored. Hence empirical rule has been adduced, based on the conditions which obtain during a period of three consecutive dry years, or years in which the rainfall is below the average.[1]

Let R be the average rainfall during three consecutive dry years estimated in millimetres, Z the number of days' storage which should be provided, then, according to this rule,[2]

$$Z = \frac{1{,}000}{0{\cdot}198\sqrt{R}}.$$

The volume of water to be stored in one day is

$$T = B + C + V - D,$$

where B = demand of the town;
C = compensation to the stream;
V = loss by evaporation from the surface of the reservoir;
D = dry-weather flow into the reservoir.

The quantities are all estimated for a period of twenty-four hours.

According to the above formula, and in England,

$$Z = 100 \text{ to } 250 \text{ days.}$$

The formula is suitable only for English conditions, where the amount of compensation-water is regulated by law, being usually one-third to one-fourth of the available supply from the catchment-basin. The formula would not be applicable for German or Austrian localities, where the amount of compensation-water is settled by free agreement with the owners of the water-rights.

3. EMPIRICAL METHOD HITHERTO ADOPTED.

The method most commonly adopted in deciding the capacity of a storage-reservoir is a purely empirical one, and depends on the consideration of the period of greatest drought only.

Any probable quantity is assumed for the capacity of the storage-reservoir, and it is further assumed that the reservoir is full at the beginning of the period of drought. By simple addition of the monthly supply to the reservoir during such a period, and sub-

---

[1] In such periods the average annual rainfall is taken as one-sixth less than the average rainfall of a long series of years.—EDITOR.

[2] If R is in inches

$$Z = \frac{1{,}000}{\sqrt{R}}.$$

traction of the supply to the town, and for compensation, also estimated for successive months, a calculation is made of the quantity in the reservoir at the end of each month for a period of a year. Should the calculation show a deficiency (the volume in the reservoir appearing as a negative quantity), the capacity originally assumed for the reservoir is increased, and the calculation is repeated.

The proceeding is an imperfect one, and is also laborious. The calculation may be shortened by assuming, as the capacity of the reservoir, the sum of all the deficiencies during the drought instead of any empirical quantity, and then making the detailed calculation for the period of a year.

But this method of calculation is open to the objection that *it is* only in certain cases that the capacity of the reservoir arrived at is sufficient to equalise the fluctuation of supply and demand, *not* only during a single drought, but during a series of periods in which the supply is deficient.

The records of the rainfall at Vienna prove this assertion. The least rainfall, and consequently the least available supply to the reservoir, generally occurs in the winter months. Thus for December, January and February, the standard rainfall is at the rate of 111 millimetres (4·44 inches). The driest winter was that of 1857–58, when for these months the rainfall was 42 millimetres (1·68 inch). In calculating the capacity of the reservoirs for the water-supply to West Vienna, this winter was taken as the basis of the calculation by the technical experts, and the empirical calculation for the driest year (1858) was made by the method explained above.

But the graphical method of the Author applied to this case, and embracing a period of thirty-seven years, during which records of rainfall were available, showed that both in the dry period, 1858–59, and in that of 1855–56, a greater storage capacity was required than in the period 1857–58. Further, the true measure of the storage required was found to be that necessary to equalise the supply and demand during the period extending from May 1855 to May 1856, as it was during that period that the greatest deficiency for the whole period of thirty-seven years was found to occur.

4. NEW GRAPHICAL METHOD OF CALCULATION.

The following is an outline of the Author's method of determining the capacity required for storage, to equalise the supply and demand during any period for which rainfall observations are available.

First the supply to the reservoir and the outflow are estimated for successive equal periods of time, usually one month, and for the whole period of time to be considered. The successive intervals of time are set off along an axis of abscissas, to any convenient scale, and the estimated inflow to and outflow from the reservoir in each interval are set up as ordinates. Connecting the points thus found two curves (or broken lines) are obtained, which may be denominated briefly as the supply-curve and the demand-curve.

The form of the supply-curve *a a*, Plate 5, Fig. 1, or curve, the ordinates of which represent the available supply to the reservoir, will in general be similar to the rainfall-curve.

The demand-curve will have a form similar to the curve *b b*. It will be the same every year, if the compensation-water is taken entirely from the reservoir. If, however, the compensation-water is partly supplied from streams which do not flow into the reservoir, as in the case of the West Vienna project, then the quantity of compensation-water from the reservoir, and consequently the whole outflow from the reservoir will vary, increasing as the rainfall decreases, and *vice versâ*.

The difference between the two ordinates at each month's end represents either a surplus (positive) or deficiency (negative), according as the ordinate of the supply-curve is greater or less than the ordinate of the demand-curve. These surpluses and deficiencies for each month are measured and entered in a Table as follows:

Column 1 gives the periods for which the successive surpluses or deficiencies are estimated.

Column 2 the surpluses or positive differences of the ordinates.

Column 3 the deficiencies or negative differences of the ordinates.

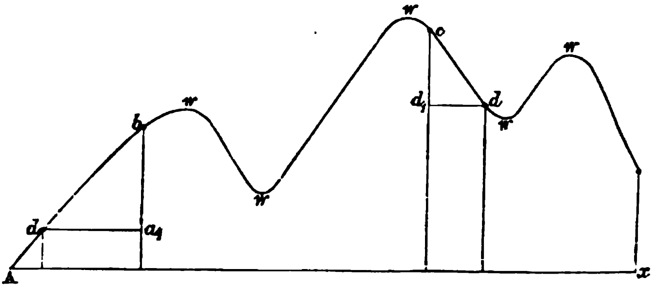
Column 4 contains, opposite each date, the algebraic sum of the numbers in columns 2 and 3 up to that date.

The following Table gives the numbers thus obtained from the diagram, Plate 5, Fig. 1.

Next, the numbers in column 4 are set off on a diagram as ordinates, the abscisses being the intervals of time as before (Fig. 2). The foot point of each ordinate is the end of the corresponding absciss, representing the period of time estimated from the point of time at which the calculations begin. By joining the ends of the ordinates a curve is obtained, which will be denominated the mass-curve.

1			2	3	4
			Differences.		Algebraic Sum or Ordinate of Mass-Curve.
			Surplus. +	Deficiency. −	
1854	End of	December	..	..	0
1855	„	January	1,048,877	..	+1,048,877
„	„	February	697,030	..	1,745,907
„	„	March	..	989,153	756,754
„	„	April	..	671,962	84,792
„	„	May	5,330,640	..	4,415,432
„	„	June	5,006,492	..	9,421,924
„	„	July	..	197,561	9,224,363
„	„	August	4,246,729	..	13,471,092
„	„	September	1,426,470	..	14,897,562
„	„	October	..	377,643	14,519,919
„	„	November	1,621,640	..	16,141,559
„	„	December	..	397,039	15,744,520
1856	„	January	828,979	..	16,573,499
„	„	February	833,518	..	17,407,017
„	„	March	..	1,368,287	16,038,730
„	„	April	..	2,651,043	13,387,687
„	„	May	395,225	..	13,702,912
„	„	June	1,548,786	..	15,331,698

FIG. 2.



The mass-curve has the following properties:—

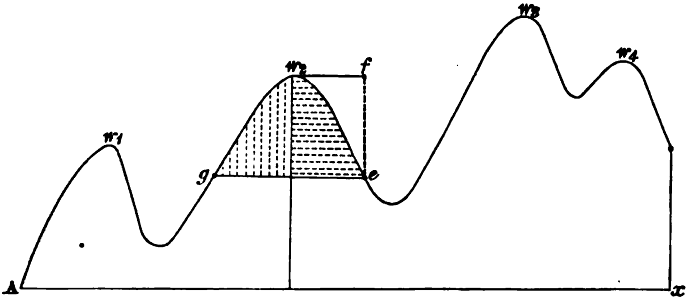
1. For the interval of time between any two points on the axis of abscisses, the difference of the corresponding ordinates is the surplus, if positive, or deficiency, if negative, during that interval. An ascending part of the curve therefore marks a period during which the quantity in the reservoir is increasing and a descending part of the curve a period during which the quantity in the reservoir is diminishing.

The crests and hollows, *w*, of the curve indicate those instants of time at which the supply and demand are equal.

2. If a horizontal line is drawn forwards at a crest, for example,

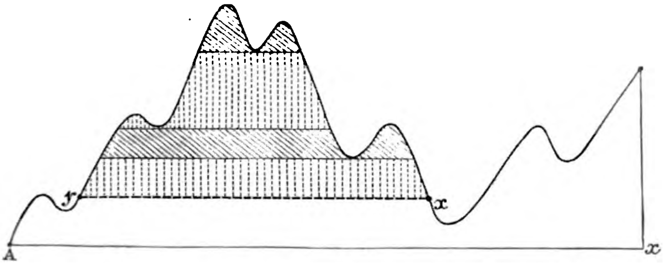
the line $w_2 f$ at w_2 (Fig. 3), the distance $e f$ of a point e on the descending part of the curve, from the horizontal line, represents the total deficiency within the period represented by $w_2 f$. To cover this deficiency there must have been previously an equal storage, and $e f$ therefore represents the amount of storage required to meet the deficiency in the period $w_2 f$.

FIG. 3.



3. From what previous point of time the storage to meet the deficiency must have commenced is found, by drawing the horizontal line $e g$, backwards from e, till it meets an ascending part of the curve. In the period represented by $g e$ the supply and demand are equal. Hence $g e$ may be termed a balancing line. This is true of any point such as x, Fig. 4, on a descending part of the curve,

FIG. 4.



that is the supply and demand are equal for the period represented by the balancing line x y, all the subordinate deficiencies being balanced by corresponding surpluses, as is indicated by the shading of the diagram.

4. In the mass-curve certain hollows t_1, t_2, t_3 . . . . (Plate 5, Fig. 5) may be selected. Lines drawn through these points mark off periods within which the surplus during one part of each

period must be stored to balance the deficiency in another part of the period.

5. The quantity of water represented by the vertical projections Y_1, Y_2, Y_3 . . . . . of the remaining portions of the ascending curve, between the lines, flows away or at all events is not required to meet the demand during the period of time considered.

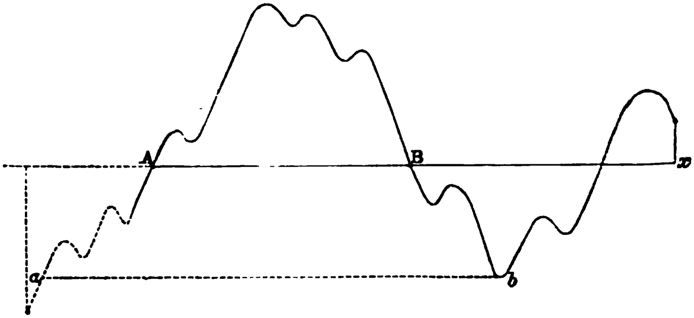
6. The vertical distances J_1, J_2, J_3 . . . . . between the lowest and highest crests and hollows in each period are the storage-capacities required to equalise the fluctuations of supply and demand during those periods.

7. Balancing quantity J. The greatest possible vertical distance J, between a crest and hollow for any one period represents the capacity of the storage-reservoir which is sought for. For if the storage-reservoir is capable of equalising the supply and demand during the period in which J is greatest, it is sufficient in all other periods.

8. The period in which this greatest value of J occurs is therefore the critical period. In the case shown in Fig. 5, the critical period is that represented by t^1, t_1; during that period all the surplus of supply over demand during parts of the period must be stored to meet the deficiency in the remainder of the period.

9. The ordinates of the mass-curve will be positive (drawn upwards) so long as the sum of the surpluses is greater than the sum of the deficiencies up to the point of time considered. When the mass-curve crosses the axis of abscisses the whole previous surplus is exhausted, and if it falls below the axis of abscisses, it is necessary to carry back the curve to some earlier point of time

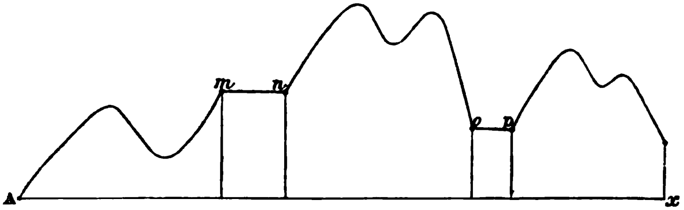
FIG. 6.



than that at which it was first started. Thus, for example, suppose the curve had been drawn from A (Fig. 6), and had been observed to fall below the axis of abscisses beyond B, then it would be necessary to prolong the curve backwards from A to *a* to make a complete investigation of the period within which the points A and B occur possible, since they fall within a period the balancing line of which is *a b*. In other words the mass-curve will remain above the axis of abscisses so long as the total supply estimated from the beginning of the time considered exceeds the total demand, and only falls below it if the demand exceeds the supply.

10. The mass-curve becomes a straight line parallel to the axis of abscisses for any periods such as *m n*, *o p* (Fig. 7), during which the supply and demand are exactly equal. Such cases, however, occur rarely in practice.

FIG. 7.



It is clear from what has been said that a single period of drought does not afford a safe basis for determining the proper storage capacity of a reservoir. That storage-capacity can only be determined with safety, by examining a series of such periods. Hence also, it is not the year in which the least total rainfall occurs which gives the measure of the storage-capacity required, but the period in which the greatest fluctuation of supply and demand happens. The limitation of the time considered to a year is erroneous in principle, because the year is in reference to the question to be solved an unessential condition. The essential intervals of time are the periods during which the supply and demand are balanced.

Plate 5, Fig. 8, represents part of a mass-curve based on observations of the rainfall in the district of the Wiener Wald, and comprises those portions of the curve only which required to be taken into detailed consideration. The curve was drawn on the method described above, and by examining it, it will be seen that a storage capacity of 4,019,330 cubic metres is required, to equalise the greatest fluctuation of supply and demand during a period extending from the end of 1854 to the end of 1859. It will be seen also that it is not in the driest year, 1858, that the greatest fluctuation occurred, but in a period extending from the end of August 1855 to the end of April 1856. It is this period, therefore, which is the critical period, and which determines the capacity required for storage.

The Paper is accompanied by several diagrams, from which Plate 5 and the woodcuts in the text have been prepared.

(*Paper No. 1875.*)

"The River Indus as a source of Supply for Irrigation-Canals in Sind."

By Charles Swaine Fahey, M. Inst. C.E.

Sind is a small province attached to the Bombay Presidency. It lies north-west of Bombay; is bounded on the north by the Bahawulpur State; on the south by the Arabian Sea and the great western Runn of Cutch; on the east by Rajputana, and on the west by Beloochistan. Kurrachee, its sea-port, has a harbour, which has been greatly improved in the last few years; it was owing to this, and also to the fact that the Indus Valley Railway was well advanced at the outbreak of the last Afghan war, that Government were able to push forward carts and bullocks from Bombay to relieve the block in commissariat stores at Sukkur, caused by the loss of a large number of camels.

Before the Indus Valley Railway was opened, all troops and stores had to be sent up the Indus from Kotree in steamers, which took from eight to ten days' travel from there to Sukkur, a distance of about 300 miles, a journey which is now accomplished by rail in one night. The Indus flows in a south-westerly direction from where it crosses the boundary of Sind to Sukkur; from thence to Halla the general direction is in an arc of a circle, the convex side being to the west. From Halla to the sea its course is again south-westerly.

Sind, away from the Indus, has a barren appearance, and the glare from the white sandy roads is painful to the eyes; but as the Indus is approached cultivation is met with, and the course of a canal can be traced for miles by the trees growing on its banks; these are chiefly babool (*Acacia Arabica*), which in Sind grow to fine trees. The Sindees are essentially an agricultural class, and up to within a few years ago were entirely in the hands of the money-lenders, who provided seed and took the produce, just giving the cultivators sufficient to live upon. The spade used by them is called a "powrah," or "Khodā," and is somewhat like an English spade, with a short handle put in at right angles to the spade. The men stand in a line about 6 feet apart, and throw the earth from one to the other.

If Sind were entirely dependent on Sindees for carrying out heavy work, very slow progress would be made; but fortunately numbers of Afghans come down for employment each cold season —say from November to April—and are capital workers, and use a spade and barrow well. From the other side of India, Cutchees come with carts; other tribes with bullocks and donkeys, to carry the earth in bags. For ordinary canal clearance the Sindee "hărees," or cultivators, have to be depended upon. They are independent and deceitful where work is concerned, and cheat in earthwork excavations; it takes a good deal of experience to detect their frauds. However, with a little tact they will, in an emergency, do work which no extra wages would induce them to do.

The total area of the plains of Sind is about 20,689,000 acres, of which about 11,578,000 consist of barren land: the population was, in 1869, about 1,578,000; so that there are about 5·77 acres of cultivated or cultivable land per head of population.

Irrigation is carried on by—

1. Natural flow ("moke");
2. Water raised by Persian wheels ("charkee");
3. Rain ("bărānee").

The principal crops are rice, cotton, jowaree, bajaree, till, (oil-seed); and these are grown in the Khureef season, from May to October, when the Indus is in flood. Gram, wheat, barley, oil-seeds are grown from November to May, principally on land that has been flooded during the inundation-season. A good deal of sugar-cane is grown in Lower Sind, but only on the banks of the Indus or perennial canals, as it requires plenty of water for eight months.

The irrigable area of Sind, about 14,222 square miles, lies between the Indus and the rocky hills on the west bank, and on the east, as far as the sand-hills to the north, and the desert and salt plains to the south. This area is intersected with a network of canals, considerably over 5,000 miles in aggregate length; these canals, with few exceptions, existed before the conquest of Sind. As the revenue of the country, and the welfare of the people depend on them, it is of the utmost importance that they should be improved as far as possible, and maintained in an efficient state.

The whole of the plains of Sind have been formed by alluvial deposit of the Indus, the old course of which can now be traced by the numerous depressions that exist all over the country. Many years ago the Indus used to divide into two branches below Tatta, that to the west being called the Buggaur, and that to the east, about 20 miles further down the river, the Suttah. The Buggaur threw off large navigable branches, but these are now irrigation canals only, and would become completely silted up but for the periodical clearance of their beds. The following extract from a report made by Lieutenant Carless, R.N., in September 1837, is interesting as bearing on this subject:—

"From the report of the natives, it appears a very high inundation sometimes occurs in Sind, which invariably causes great alterations in the lower part of the Indus. It is said to occur once in half a century. In 1819 (the year in which a great earthquake was experienced in Cutch), one of these floods came down, and the river was several feet above its usual height during the swell, and the strength of the current was much greater than in ordinary seasons, while villages were swept from the banks, and in many parts of the country the crops were completely destroyed. On this occasion the river altered so much about the part where the Setta (? Suttah) was thrown off, that a larger body of water than usual was forced into that stream, and it increased in size considerably. The change became greater every successive year, until at last the main river turned into the Setta and abandoned the Buggaur altogether. . . . The rulers of Sind had a fleet of fifteen ships stationed at Shahbunder (the King's Port), which owes its name to that circumstance, and it is mentioned in the histories of the country that they sometimes ascended the river as high as Tatta. The line of route they pursued from the sea to Shahbunder is accurately pointed out by the natives. They entered by the Reechal creek, the only accessible mouth, and passing into the Hajāmree through what is now the Kedewaree creek, ascended that river to a point about 10 miles above Vikker."

The Indus water is heavily charged with silt; and on this subject the Author cannot do better than quote from a Paper written by Colonel (now Major-General) Tremenheere, R.E., C.B., when Chief Engineer in Sind:—

"The mean result of twenty-nine observations taken at Sukkur is that the silt amounts to $\frac{1}{237}$ (or 0·00422) part of the water by weight; but, adding the two observations taken on August 3rd and 9th, it amounts to the $\frac{1}{227}$ (0·00441).

"The experiments on the 3rd and 9th are remarkable for agreeing so closely with each other; the proportion of sand and clay in the two combined being exactly one-half of the deposit.

"The result is that the river water contains about $\frac{1}{210}$ (0·00476) parts of its weight of matter in suspension. One half, or $\frac{1}{420}$

(0·00238) part, consists of clay, and the other half of sand of specific gravity varying between 2·430 and 2·688, or from 150 to 168 lbs. per cubic foot."

General Tremenheere drew the following conclusions from his observations, which were carefully made by himself, the water being obtained at different depths by means of a brass bottle (made to open and close under water at pleasure), lowered to the required depth by an iron rod:—

1. "That water in canals, deriving their supply from branches, separated from the main river by islands covered with brushwood and long grass, contains a comparatively small amount of material in suspense.

2. "That water in canals, whose heads are in the main stream, where the velocity of the current is not exceptional, may be expected to contain an amount of sand and silt equal in weight to about $\frac{1}{300}$ of the weight of the water, and that $\frac{1}{3}$ of this, or $\frac{1}{900}$, will be deposited in the bed of the canal, within a distance of its mouth which will vary with the velocity maintained in the canal.

3. "That canals, constructed so as to derive their supply from a part of the river where the section is contracted, and the velocity is much augmented and exceptional, will probably contain sand and silt varying from $\frac{1}{200}$ to $\frac{1}{250}$, the half of which, or $\frac{1}{400}$ to $\frac{1}{500}$, will be heavy sand, which will be deposited in any canal in which the velocity is as low as $1\frac{1}{2}$ foot, or 2 feet per second."

Again General Tremenheere says: "It may be interesting to consider the amount of solid matter which must be carried down by the Indus. The discharge of the river at Sukkur during the inundation, with a depth of 13 feet on the gauge, amounts to 380,000 cubic feet a second; on the supposition that only 250,000 cubic feet are discharged into the sea for one hundred days, and that the water so discharged contains the same proportion of silt as it has been ascertained to hold at Sukkur, namely, $\frac{1}{250}$ part, the total quantity will be found to exceed 119 millions of cubic yards, or sufficient to cover a surface of $38\frac{2}{3}$ square miles with a deposit a yard in thickness. It may be observed that the actual quantity of sand and silt moving forward with the current must be the same in each section of the river. The rule which is applicable to the uniform discharge of water in different river sections, must apply equally to the solid matter held in suspension. Where the velocity is exceptional, as in the narrow pass at Sukkur, the water and sand are more intimately mixed, and the surface water will contain a larger proportion of sedimentary matter than elsewhere; but the total quantity of solid matter is no more affected by the additional velocity than is the volume of water discharged by the river." General Tremenheere came to the conclusion that "in the low season, the proportion of silt by weight in the whole discharge of the river, was as nearly as possible half that contained in it during the inundation season, or 16·6 parts in 10,000."

The rainfall in Sind is so very small that it is entirely left out of all calculations connected with irrigation works; it is of more frequent occurrence and heavier near the sea coast than in Central or Upper Sind, where dust-storms apparently take its place. These dust-storms seem to do as much good as thunder-storms do elsewhere. Some time before a dust-storm comes on there is the same still, oppressive feeling, which is experienced before a thunder-storm; and, after it is over, the air is cool and fresh, and to a great extent purified from insect life. The rainfall in one year, at a place called Meerpoor Buttora, in Lower Sind, about 70 miles from the sea, never exceeded 27·34 inches from 1867 to 1879. This amount fell in 1869, and nearly one-half in the month of July; in 1874, 12·4 inches fell; in 1878, 16·17 inches; while the fall in the other years varied from 1·34 inch to 9·66 inches. The average rainfall for the thirteen years, including the two very exceptional ones, may be set down at 8 inches.

Some lands, out of reach of the Indus water, are classed as Bārānee land (*i.e.*, land watered by rain), on which wheat is grown when the rainfall has been sufficient to well saturate the soil; but the area of such land is very small. The principal agricultural operations are carried on from June to September, when the Indus is in flood, and it unfortunately so happens that the heaviest rainfall takes place in July and August, when, as the Indus is then at its highest, it does more harm than good. Just at this time several of the most important canals are carrying more water than can be used, so the cultivators close their irrigation channels; the main feeder is sometimes unable to carry on all the water, and so breaches occur, with disastrous results. This could be partially remedied by regulating sluices at the heads of, and at intervals along, the main canals, and, in fact, is being done where it can be shown that a sufficient return will be obtained for the outlay.

The rainfall, then, being worse than useless for the general purposes of cultivation, the only other sources of supply are the river Indus and well-water. The latter it is not intended to notice, since near the river the water of the Indus would be used, owing to the fertilising silt in it; while, at a distance of 6 to 10 miles from the river, well water is generally brackish, and therefore unfit for irrigation, as, owing to the rapid evaporation, a crust of salt would be left on all land watered by it. However, salt land, known in India as "Kuller," or "Reh," is very suitable for rice crops, provided an ample supply of sweet water can be obtained to flood the land.

The length of the Indus, from the point where it crosses the most northern boundary of Sind to the sea, measured along its channel, is about 550 miles; whilst the distance by road between the same points is about 420 miles, and the direct distance about 330 miles. The course of the Indus is constantly shifting, owing to the main current changing its "set"; and it may be said that, to the east, the river's extreme bounds are unascertainable. The width of the Indus varies very much, according to the nature of its banks. At Sukkur there is rock on each bank, and several rocks crop up in the bed of the river. In the centre of the river, between Sukkur on the right bank and Rohree on the left, an island, called Bukkur, divides the river into two passes. The current in these passes was sometimes so strong, during the inundation season, that the steamers of the Indus steam-flotilla could make no head against it. The velocity has been slightly reduced by widening the rocky channel in the Sukkur pass.

Rock is next met with at Sehwan, where the western range of hills, dividing Sind from Beloochistan, abut directly on the Indus; there the river at times sets in with great force against the rocks, making navigation very dangerous. Below Sehwan, although the hills are within a few miles of the right bank of the river the whole way down to Kotree, no rock is met with, in either bed or banks, until Jerruck is reached, where there is rock both in the bed and on each bank. From Jerruck to the sea only two small rocks crop up near Kotree Allāruckia.

The width of the Indus can be well defined only in three places out of the 550 miles of its course through Sind, namely, at Sukkur, Kotree, and Jerruck; for, although there is rock at Sehwan, it is only on one bank, and the width is constantly varying. The bed and banks at Kotree are clayey, and have remained permanent for many years. The width of the pass between Sukkur and the island of Bukkur is from 350 to 400 feet; the Rohree pass is about 600 feet wide; and at Sehwan, Kotree, and Jerruck, the width is about 1,500 feet.

The surface-fall of the Indus in flood is about 5·3 inches per mile from the northern boundary of Sind to the sea. The surface-velocity in the Sukkur pass, during the inundation season, was observed to be 9½ feet per second, 4 to 4½ feet immediately above the pass, and 3 to 3½ feet clear of the pass above. The depth of water in one part of the pass exceeded 40 feet. The discharge of the Indus at Sukkur during floods is 380,000 cubic feet per second, while in December, when the river is at its lowest, it is about 68,000.

During heavy rains a large quantity of water is discharged into the Indus by two torrents, the Sunn and the Barun; the former falls into the Indus below Sehwan, and the latter about 4 miles below Kotree. They are about 1,500 feet wide, and where the main road from Kurrachee to Sehwan crosses them, the greatest depth of water during floods does not exceed 5 or 6 feet. The declivity of the beds of these rivers is very great, from the hills which they drain being close to the Indus. To show the effect of their discharge into the Indus, it may be mentioned that during a high inundation the Barun, flowing into the Indus almost at right angles, caused a temporary rise of the river at Kotree, 4 miles up stream. When these rivers are in flood, the Indus is diverted from its natural course immediately opposite them, and flows down perennial by-channels. When strong winds are blowing up stream, boatmen take advantage of these channels to avoid the rough water in the broad reaches opposite the rivers.

A strong wind up the Indus stops the navigation of all native craft down the river. The Author has known one of the Indus steam-flotilla vessels, when coming down river, having to put into the bank for fear of breaking its back in the rough water.

The tide up the Indus is felt as far as Tanka, 90 miles from the sea, and it causes frequent changes in the mouths and creeks of the Indus. A creek, down which fresh water used to flow, is often deserted by the Indus, thus throwing the land along its course out of cultivation; still the result is generally to benefit some other part of the delta.

The conditions for a good canal-head are:—

1. A permanent bank.

2. A tolerably straight channel 1 mile above and below the canal-head.

3. A low velocity in the river.

4. That the mouth of the canal should be at right angles to the stream.

5. That the river at the site of the canal-head should always flow within soil.

6. That the water in the river should attain a given height by

a given date, and remain at that height for a certain number of days.

The Author proposes to explain how far the Indus complies with these conditions, and what is the result of its not doing so.

1. The banks of the Indus are continually changing, except at the few places already mentioned; the river receding from or cutting into them, in places, as much as 4 miles in two or three years.

2. The river is not straight, the current bounding first to one bank and then to the other, eroding it on one side and throwing up a silt bank on the other.

3. When the river is cutting into the bank the current rushes past the mouth of a canal at the rate of from 4 to 6 feet per second; at other times the velocity is so slack that the supply to the canal is cut off by silt-banks and bars.

4. With the river constantly changing its course, the entrance to a canal can never remain at right angles to the river, even when it was originally made so.

5. As the slope of the country is away from the river, and as the "set" of the current often makes a difference of 2 feet in the surface-level of the water in the same cross-section, this condition of the river, always flowing within soil, cannot be relied on.

6. The Indus is so uncertain and fickle that cultivation in Sind on the inundation-canals is a mere lottery. The river rises before water is wanted, and just as the land is ready for water, it falls. Again it rises at the proper time, gives every hope of a good season, but falls before the crops have arrived at maturity; then rises again when too late and water is not wanted, and floods the country; and where crops, owing to local favourable circumstances have been so far successful, these are destroyed.

To illustrate the action of the river in eroding and receding from a bank, two canals in the Kurrachee Collectorate, the Buggaur and the Kulree, may be cited. The former was an old arm of the river (already alluded to) which is about 80 feet wide at the mouth and carries in flood 12 to 14 feet of water. The latter was also a branch of the river, and used to take fresh water to Ghārā, near Kurrachee, now the site of a salt-water creek. The Kulree is about 30 feet wide at its mouth, and in flood is about 10 feet deep. Fig. 1 is a sketch showing the course of the current when it is setting in at the mouths of the Buggaur. The oscillations of the river are so regular, that when it is setting in at the mouth of one canal, say the Kulree, it is known for certain that it will be setting in at the mouth of the Buggaur also. The Author has observed this as regards the Indus in Upper as well as Lower Sind. The river then goes on eroding the bank at A A″, and throwing up silt banks at B*. Trees, bushes, &c., falling into the river are dropped or grounded on the sandbanks. The river may go on cutting into the bank at A′ for five or six years; the whole time, however, the "set" is gradually working down stream towards A″, until its action ceases at A A′ and works along the line denoted by the arrows *a a*. The upstream side of the shoal B is being cut away, and the deposit is extending downstream towards A, until at last the Buggaur is fed by the backwater (Fig. 2). This process goes

Fig. 1.

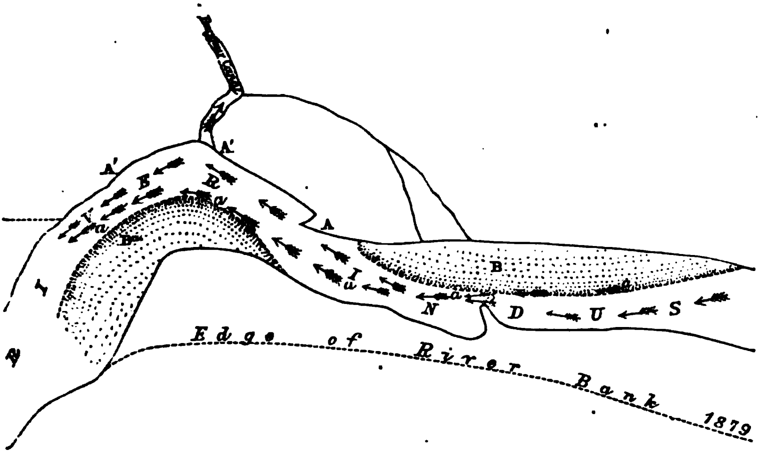
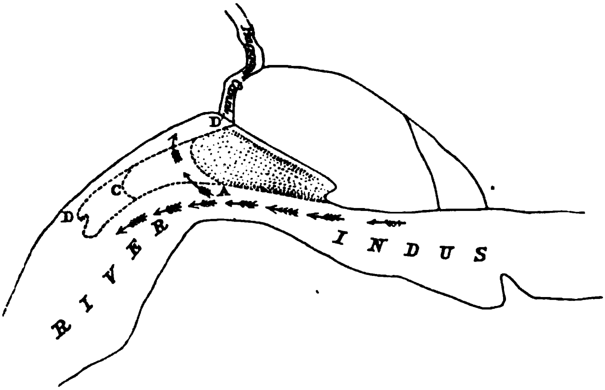


Fig. 2.



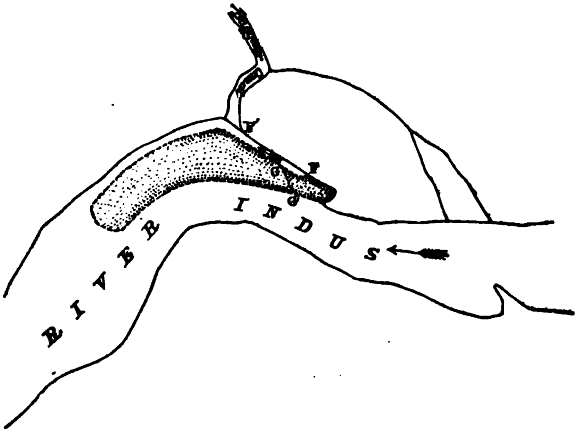
on until, by the end of another inundation season, the downstream end of the shoal is at C, and, during floods, the Buggaur is fed by the suck down its channel over the sandbank and from the direction of C. In about five or six years matters become as bad as they can be for the Buggaur (or any other canal similarly situated). The upstream side of the shoal is silted up almost to high-flood level, and the canal has to draw its supply up the backwater channel D D′, thus not only losing the 2 feet head of water it had when the river was unobstructed at its mouth, but also the difference in surface fall of the Indus from A to D. At this stage, petitions come in from all sides from cultivators concerned, for a new mouth to be cut to the canal, drawing its supply from above the upstream side of the shoal. In the case of a large canal like the Buggaur, the request cannot be complied with, as by the time the project is prepared and the work carried out, the old mouth may be better off than the new one. No effectual remedy can be carried out within a reasonable time and reasonable cost; all sorts of plans are tried, such as making an opening through the shoal from A to D, so as to admit any early supply to the canal; but this cut silts up as soon as the sandbank is topped, and water ceases to flow in the canal on the first fall of the river. Floating groynes to train the set of the current against the shoal have been tried with some success, but generally these expedients are out of the question, when there is only one executive engineer and four or five overseers to about 6,000 square miles of country, with local funds works, roads and buildings to attend to, as well as hundreds of miles of canals.

No suitable dredge to work ahead of itself has ever been tried in Sind for keeping a channel through the sandbank open.

When the worst has happened, matters begin to mend in the following manner (Fig. 3). There is always a shallow channel at F F′, caused partly by the draught down the canal. The water, during an inundation, breaks over in one or two channels G G′. Then it becomes necessary to clear these channels in the low-water season, and to continue the clearance along G F′. In the course of time the river makes a channel as far as F′, and the supply to the canal is improved. But all the time this process has been going on, silt has been dropping in the canal, extending perhaps 1½ mile or more along its course. Large sums have to be spent in brushwood training groynes to increase the scour in the canal, and also for removal of silt by manual labour. In cases like the above, a powerful dredger that could cut a channel for itself and deposit the material dredged at a safe distance on either side, would be invaluable; but at the same time it must be of a manageable size, and be able to make at least 4 miles an hour against the current in the Indus. Mr. Barnes, Superintending Engineer, Bahawulpur State, invented a diamond-shaped dredger, the bucket ladder extending across the bows; but the Author believes a tug was required to move it from place to place.

The river takes about twenty years to perform the whole process described above, namely, from the time it commences to cut the bank at one particular point, to leave this point and again attack it. There are exceptions to this rule, as where there is a particularly sharp bend; here an unusually high inundation causes the river to force a new course across the bend, but even then the river has a tendency to return to the more circuitous course at the termination of the inundation.

Fig. 3.



These oscillations of the Indus are as regular as the waves produced by shaking a rope fastened at one end, the rope at the same time moving forward in the direction of the waves.

A clay spur or a groyne may to some extent protect a bank, and throw off the current locally; but the set of the current will not leave this point until the proper time comes. Hence some idea may be obtained of the great force with which the water acts at any point of resistance. Unopposed, it simply tumbles the bank in in large lumps and flows steadily on, but opposed, it is headed up in the bight of the river, causes dangerous eddies and back-waters. The Author, never having seen the Indus north of Sind, does not understand what causes the river to make such regular periodical changes in its course, and he has never heard of any theory to account for it.

It is impossible, unless the banks and beds are firm for some distance above and below the mouth, to maintain a straight reach from which to take off a canal. The Author was told by an Inspector-General of Irrigation in India, that he thought it possible, first, to make a straight reach well protected along its bed and banks, and then to turn the Indus down it. Setting aside the enormous expenditure such a work would involve, it is doubtful whether the river could be forced to pass down the artificial channel, and if it could, whether it could be kept there. The Author, from his experience of the Indus, extending over a period of fifteen years, is confident that it could not be done.

Again, whether the banks be permanent or not, if the velocity across the mouth of a canal exceeds the proposed velocity *in the* canal, the result must be that the latter will soon silt up. Of course some silt will be deposited in all but the largest canals, *in* which a high velocity can be kept up; but if a canal is led off from a point in the river where the velocity is from 5 to 6 feet per second, the water at this point will have its full proportion of silt in suspension, and the heaviest part of this silt, namely the sand, which the above velocity was able to keep in suspension, will drop in the mouth of the canal, where the velocity is suddenly reduced to about 3 feet per second. This fact admits of no dispute: it is proved every year in the Sind canals. If a canal is in fair order, that is, if it has a properly regulated width and bed-slope, the sandy deposit will be distributed along the upper third of the canal, the heavier sand in the first mile or so: the finer, lower down, and the clay at the extreme tail, while the central portion will seldom or never require clearing. Although the velocity *in* the canal is not sufficient to carry on the sand, it is sufficient to carry on the clay, and if only escapes could be provided at the *tails* of all canals—which is not practicable in Sind—there would be no clay to be annually removed.

Assuming that permanent banks with a straight reach can be obtained, then the velocity across the head of the canal must not be too great.

A shelving bank with the set of the current in mid-channel is a good one, because the velocity over the bank cannot be very great, although it may be sufficiently strong to carry on the silt. But this is seldom the case, and silting goes on until a steep bank is formed out towards the centre of the stream, and the velocity is sufficient to carry on the silt.

Some canals have not sufficient draught down them to keep open a channel through sandbanks deposited in front of them by the Indus, and such canals draw their supply over the banks. This is very well as long as the surface of the banks keeps below the beds of the canals, notwithstanding there is some loss of head, and if jungle has commenced to grow over the banks, as it soon does, the water enters the canals tolerably free from silt, thus reducing the cost of annual clearance.

The outer edges of the banks, however, often become so silted up that water is unable to flow over them towards the canals, except when the river is very high. When this is the case, a new cut from the main stream of the river to the canal has to be made, frequently more than a mile in length, and as generally there is a good deal of jungle to be cut before the levels can be taken, all this takes up a considerable time out of the short working season.

Whether the banks of the river are permanent or not, it is advisable that the highest flood-level should be below the surface of the land, otherwise embankments both up and down stream become necessary, or the canal head would be taken in flank by floods. Occasionally, when a mouth to a canal was originally cut, the water was within soil, and no embankments were required; but as the country, as a rule, slopes away from the river, embankments have afterwards become necessary, owing to the encroachments of the river reaching a point inland below the flood-level, and owing to the fact that the "set" of the river being against that bank, would make a difference of 2 feet in the level of the flood-water. It also becomes necessary to construct a masonry sluice across the canal in a line with the embankments, or the floods would rush down the canal and breach its banks.

It is always a difficult question to decide how far inland the sluice and embankments should be made; if constructed too near the river, they may be carried away with the river bank; on the other hand, if they are made far inland, they must be proportionately more costly, and more land has to be given up to the floods.

As the Indus is so unreliable as to rise and fall at the required times, it may be considered that a canal with a good head of water, running within soil for the first few miles, then through lands gradually sloping down towards the tail of the canal and embanked on each side, would meet the difficulty. Many such canals exist in Sind; but, excepting new works, there is scarcely an instance where the water is fairly under control. The Pinyaree, a canal in the Shahbunder division of the Kurrachee Collectorate, may be given as an example. With the water of the canal properly under control, there could scarcely be a better one, as far as relative levels of land and canal water go. The main canal is 73 miles long, and it has numerous well distributed branches, aggregating several hundred miles in length. For the first 12 miles the water is within soil; but from this point the land gradually becomes lower along the main feeder, as well as along the branches. Towards the end of the main feeder the land gets higher again; and so, in former days, to raise the level of the water and prevent it running to waste into the sea, a large earthen bund, or dam, was thrown across the channel. Several canals, some with rudely constructed head-sluices, others without, were taken off above the bund, around which an escape, unprotected in any way with masonry, was excavated. During a moderate inundation all worked well in the Pinyaree and its branches; but the moment the water-level of the Indus got high, there was no means of reducing the supply at the head. The zemindars closed their branch canals, and the main feeder became choked.

The water had no difficulty in obtaining outlets through the rudely constructed embankments along the canals, and flooded hundreds of acres of cultivated land to such an extent that it took two or three years for the land to recover. The escape round the bund, at the tail of the Pinyaree, was generally kept closed by a small bund at its head, which had to be frequently breached to save the land above from floods. In course of years this passage became so large and deep, where the bund across it was breached, that a new cut had to be made. So matters went on until there was no safe place to make an opening, and the construction of an escape-sluice could no longer be deferred.

Some years ago a project was prepared for regulating-sluices to the main feeder, as well as to all its branches, and also for an escape-sluice; but, because a return of 5 per cent. on the expenditure could not be shown, the project was not sanctioned. Before the Author left Sind he prepared and obtained sanction for a revised project, and was able to show a return of over 5 per cent. from saving the annual cost of making good damages by floods, remissions of revenue, &c.; a portion of the project was carried out under his supervision.

At present the cultivators draw water off through the side-embankments to feed small field-irrigating cuts. The *Author* pointed out that it was advisable, in all cases of high-level canals, to stop this system altogether, and to make small branch canals, with strongly constructed head-sluices, on each side of the main feeder, at frequent intervals along its course, parallel to it, so that water would be drawn from the subsidiary canals, instead of directly from the main feeder.

This system, known as the "Rājbahā" system, was proposed to remedy the defects in the existing system. The reason why this Rājbahā system could not be carried out was, that the cultivators had certain proprietary rights to the water and to meddle with the banks of the canal that could not be interfered with by Government without a special Act.

To properly economise the water, and to have it fully under control where it flows above soil, regulators, or sluices, are required across the main feeder at about every 6 miles. It is not sufficient to have a head and tail regulator, with a sluice-bridge at the head of each branch.

The usual method adopted in Sind for regulating the supply of water through sluice-bridges, is with balks and needles. It has answered very well, being simple, and having nothing about it to get out of order. The needles are of teak, generally 4-inch scantling, and are placed in front of the balks, which are of Babool wood, either from the roadway or from the top of the projecting piers. Where it is necessary to provide for the passage of boats, horizontal beams are used, the ends sliding in grooves in the projecting piers. The ends of each beam are provided with folding handles of iron, and let in flush with the surface of the beam, with round holes in them, into which iron hooks fastened to a rope may be fixed; two men on each side can easily raise a beam. This plan was first suggested by Mr Price, M. Inst. C.E., in his project for the Sukkur and Shadadpoor Canal-Regulators. In several instances waterways 16 feet wide had to be provided for the passage of boats. In one instance the Author made the boat-opening next to the left bank abutment, on which he fitted up a Henderson's derrick, by which the beams were easily raised and swung round on to the canal bank.

In the case of the escape-sluice for the Pinyaree, it was necessary to prevent all leakage, and so teak gates were fitted, raised by means of a pair of screwed bars, one at each end of the gate, with brass nuts working on the screws, and secured by collars to beams at the top of the parapet of the bridge. The nuts were worked with double levers, which could be removed when not wanted, so that the gates could only be raised or opened by the persons in charge.

About twenty-seven years ago Government decided to try how a high-level perennial canal from the Indus would answer. A spot just above the Sukkur pass was selected for the head of the canal.

The head-regulator was designed to be inland, with a scouring-channel in rock, with head- and tail-sluices, the object being to free the water, to a certain extent, of the Indus silt. By opening the sluices occasionally, it was proposed to keep the scouring channels clear. The canal was to have been about 75 miles long, with a bottom width of 35 feet at the mouth, side slopes of 1½ to 1, and to carry 6 feet of water, the bed being 6 feet below zero of the water-gauge at Bukkur. The highest reading of this gauge is 13 or 14 feet, so that during inundations there would have been about 19 to 20 feet depth of water at the head of the canal. Here then, it might be supposed, was one of the most favourable sites that could be obtained for a canal, the head and bank being permanent, a head of water obtaining in floods of 13 to 14 feet, and a bed 6 feet below the low-water level of the Indus. But what was the result? The then chief engineer in Sind was opposed to the canal from the commencement, and showed, by experiments on velocities and silt of the Indus at the proposed site, that the canal could never be perennial, as, on the inundation subsiding, 6 feet depth of silt would be found in the bed. Eventually a head-regulator was erected in the river, groynes were run out from the bank above the canal-head, a river-wall was built connecting the groynes with the regulator, and a second mouth was excavated about 4 miles higher up the river, falling into the main canal about 4 miles from its head. This new mouth drew its supply from an old partly silted-up bed of the Indus, in which the water was tolerably clear, owing to its being filtered through jungle. The canal drew its supply from the main head during inundations, and from the second mouth when the river was low. The latter, however, can be but of temporary use; for, several years ago, the channels, which fed the old bed of the Indus, became silted up, requiring to be dredged. The river will, sooner or later, either entirely silt up the old bed, or again go back to it; in the latter case, the second mouth up the river will be in a worse condition, as it has no rock to protect it, than the main mouth at Sukkur. This case shows how utterly unsatisfactory the Indus is as a source of supply for canals. Here, the result of putting the canal-head in the river was to draw in water so heavily charged with silt that the canal became choked up. If the head had been left as originally designed, the river would probably have deserted the mouth of the scouring channel at times, and the supply of water to the canal would have been cut off. This difficulty was overcome by the new head, as it was placed in a part of the Sukkur pass that does not silt up; but the change was made at the expense of the whole project, which could afterwards no longer be considered a perennial canal. Again, the main canal was eventually made to carry 12 feet depth of water, instead of 6 feet, and to feed a number of old canals; so that, instead of a perennial high-level canal, as first proposed as an experiment, the Sukkur canal, as completed, is little better than a good inundation canal.

A little below Kotree Allāruckia, there is a branch of the Indus, called the Kokāwāree, down which the main volume of the river used to flow to the sea; the branch going to the right, and called the Oochta, meaning by chance, was then a channel scarcely as large as the Buggaur, say from 80 to 100 feet wide, but is now the main channel. The natives say that the Kokāwāree first began to become shallow, and the Oochta to become enlarged, in 1847, and to make a decided set down the Oochta in 1859; but, however this may be, about the year 1862 a rubble-stone groyne was made on the right bank of the Indus, with a view of turning the set of the river down the Kokāwāree. As there was no firm bottom for the groyne, the Indus washed it away. The Kokāwāree first commenced to silt up at its mouth, *i.e.*, where it leaves the Indus, a bank of sand being thrown across it. This was the commencement of the complete ruin of the channel, for the supply of water down it being insufficient to head back the tidal water, it fast became silted up, and all the land dependent for its water supply on this branch of the Indus was thrown out of cultivation. The Executive Engineer in charge of this work before it came into the Author's charge, proposed making a narrow cut through the sandbank at the mouth of the Kokāwāree, and to turn its channel into a large lake, as was done by the old rulers of Sind in the case of the lower portion of the Pinyaree called the Goongra; but in that case the lake was at the tail of the channel, and after the water had become tolerably clear of silt in a course of over 70 miles, while in the former case the lake would have received at once the heavily charged silty water of the Indus. The proposal was therefore not adopted. The supply was eventually restored by excavating a cut, 9,000 feet long, 50 feet wide, and from 8 to 12 feet deep, varying with the surface irregularities of the land, leaving the Indus 1½ mile below the mouth of the Kokāwāree, and falling into this channel at a distance about 8 miles along its old course. It was completed at a cost of a little more than 10,000 rupees just as the Indus rose; in fact the depths of excavation were reduced for fear of the whole work being flooded before completion. About two months afterwards, the Author, with the Superintending Engineer in Sind, passed through the cut in the Commissioner's steamer. The scour on the rise of the river had very considerably widened and deepened the channel. The large quantity of silt carried down was nearly all deposited in the old bed of the Kokāwāree above where it was joined by the new cut, and along the broad portions of the channel below.

The supply to the old channel of the Kokāwāree was so effectually restored by the above means, that next year the cultivators were urgent in their petitions for its embankments to be repaired. The Author's predecessor accounted for the Kokāwāree silting up, and the Oochta being enlarged, by attempting to show that the course down the Kokāwāree to the sea was shorter than that down the Oochta, and in consequence the tides acted with more force up the former channel than the latter. The Author, however, ventured to express an opinion that if this were the case, the reverse would have been the result, namely, that the tides, assisted by the Indus water down the Kokāwāree, would ebb quicker than down the Oochta, and would cause such a scour down the former, especially at low tides, as would effectually remove any silt deposited at high tide.

Although the tides up the Kokāwāree may not have been the primary cause in turning the main supply down the Oochta, there can be no doubt that, when once the supply was reduced, they had a great deal to do with the final and rapid silting up of the mouth. The Indus had every inducement to flow straight on in an uninterrupted course down the Oochta, instead of turning round the bend in the mouth of the Kokāwāree. Reverse matters, and consider that the tide up the Oochta was of the same force and duration as that up the Kokāwāree, and assume that the tide up the latter was equal to that up the former, and the Author maintains that the main volume of the Indus would still have flowed down the Oochta, where, owing to the channel being straight, the impetus is unbroken and has more power to overcome the tides.

If the theory that the course down the Kokāwāree, being the nearest to the sea, was the primary cause of the channel silting up, the very remedy carried out should have made it worse, as it still further shortened the distance. Up to the time of the Author leaving Sind, five and a half years after the cut was made, the new mouth to the Kokāwāree had shown no signs of silting up.

There are many instances where the tide, having found its way up the bed of a canal in the delta of the Indus, owing in the first instance to the bed having been scoured out by land-floods, from the overflow of the river higher up, has so enlarged the canal as to necessitate its either being abandoned, or, when taken in time, being dammed across at the tail to exclude the tides. The Author has observed that, where a bund or dam has been put across a branch tidal channel to intercept the tides, the branch channel on the tidal side silts up; but if the tides are allowed to expend themselves up the bed of the branch they invariably scour it out. This is due to the water in the latter case having a longer time to act on the bottom, the ebb in the main channel being quicker than in the branch.

If the distance to the dam were sufficient, the channel would be scoured out, except for a certain distance in rear of the dam, depending upon its distance from the main channel. As an instance of this, the creek below the bund across the Goongra may be noticed; although the bund has been made over half a century, the ebb and flow up the creek are still able to keep open a channel in it for boat traffic in all tides from a point 4 miles below the bund to the sea, a distance of about 60 miles.

The floods from the Indus along the lower part of its course in the delta, spilling over into creeks parallel to the river, sometimes scour out these creeks, and by retrogression of levels form mouths to them from the Indus.

Given a clear channel with a good fall, the quantity of solid matter to be removed by scour, for the enlargement of a small creek into a large mouth or arm of the river, is speedily effected, as is proved in the instance of the Oochta, which, as already stated, was only about 80 feet wide thirty years ago, and is now 1,500 feet, and in places even more.

In this Paper the Author trusts he has succeeded in showing the almost insurmountable difficulties met with by engineers in dealing with canals drawing their supply from such rivers as the Indus.

The Paper is illustrated by several diagrams, from which the cuts in the text have been prepared.

(*Paper No. 1882.*)

"Various Methods of Determining Dimensions of Iron Structures."

By Dr. James Weyrauch, Professor at the Polytechnic of Stuttgart.

(Translated by George Rudolph Bodmer, Assoc. M. Inst. C.E.)

In a former Paper,[1] the Author promised subsequently to give a short demonstration and comparison of those methods which have recently been proposed of determining dimensions, based on the assumption of a variable strength. This review it is intended to give in the present Paper. The development of each method must of course be confined to the essential ideas involved. For purposes of ready comparison this will be followed by the determination of the limiting stresses per unit of sectional area allowed by each, and, in conclusion, a number of examples and tables will be given. Some of the methods shown have obtained a footing in practice, others contain valuable ideas for further development, and all are of interest as steps forward on the road towards a rational method of determining dimensions. When it is once recognised that the strength of materials has not the same value in all the various circumstances in which the loads are applied, a method different to that hitherto used must gain ground. Those who cannot approve any of the proposed methods will, perhaps, by a knowledge of them, be stimulated to achieve something better.

The general points of view explained in I. to IV. of the Author's former Paper will be assumed as known, and the nomenclature and notation there introduced maintained. Hence t denotes the *statical breaking-strength* per unit of area (developed by a static load once applied), u the *primitive strength* (breaking strength for stress of one sense alternating with zero), s the *vibration-strength* (strength developed by oscillations, where there is an alternation of stresses of equal intensity in opposite senses), a the *ultimate working-strength* (the breaking strength under the particular circumstances of loading), d the difference of the limiting stresses, and b the admissible stress; the word stress being an abbreviation for "intensity of stress," unless otherwise indicated.

For the sectional area F, let ϕ denote the ratio of the numeri-

---

[1] Minutes of Proceedings, vol. lxiii., p. 275.

cally smaller to the numerically greater limit of stress (ϕ being positive when both are tensile, negative when one is tensile and the other compressive, ϕ_0 the ratio of the stress produced by the fixed or "dead" load, if any, to the numerically greater limit of stress, and B_0, B_1, and B_2 the numerical values (without signs positive or negative) of the fixed load,[1] and the two limiting values of the variable or "live" load.

Then there follows for tension only or compression only, if max B and min B denote the numerical values of the upper and lower limits of total load,

$$\phi = \frac{\min B}{\max B} \qquad \phi_0 = \frac{B_0}{\max B} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

and for alternations of tension and compression, when max B and max B′ denote the values of the numerically greater and smaller limiting total loads respectively,

$$\phi = -\frac{\max B'}{\max B} \qquad \phi_0 = \pm\frac{B_0}{\max B} \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

In the latter expression the upper or lower sign is to be taken according as the dead load produces stress of the same or opposite sense as the upper limit of live load.

If the admissible stress *b* be known, then

$$F = \frac{\max B}{b}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

as in equation (2) of the former Paper.

Where in the sequel numerical values occur, the stresses *b* are given in kilograms per square centimetre, the values of F in square centimetres.

I.—Gerber's Method.

The first work on the admissible stresses for iron and steel based on Wöhler's experiments, was written by Gerber, the manager of the South German Bridge-building Establishment, in 1872, adopted by the Bavarian Government, and published in 1874.[2]

Every piece of a square unit of sectional area would be destroyed by a static load producing stress of intensity = *t*. The same result

---

[1] "Load," as in the former Paper, means the total amount of the external force, tensile or compressive, as the case may be, applied to the single bar or piece under consideration.

[2] Gerber, "Bestimmung der zulässigen Spannungen in Eisenconstructionen." Zeitschrift d. bairischen Arch. u. Ing. Vereins, 1874.

may be attained by a load constant only as to one portion c, and as to the other d acting by numerous temporary repetitions; hence the difference of stress d is equivalent to a certain static load τd, and there follows

$$(a) \qquad c + \tau d = t = \delta d,$$

where δ denotes a coefficient, determined by the preceding equation.

If for a piece of any Section F, the static calculation give a fixed load, B_e and a limiting value B_v of the live load, then these loads might by means of the equation

$$(b) \qquad B_e + \tau B_v = B_r = \delta B_v$$

be reduced to a static load, if only τ or δ were known, and the requisite section would then be found by

$$(c) \qquad F = \frac{B_r}{b_r}$$

where b_r is the admissible stress per unit of area for a static load.

The hypothetical loads B_r Gerber calls "reduced forces."

As Wöhler has determined the statical breaking-strength t for certain materials, and also the possible differences of stress d for various initial stresses c, by substituting these special values c, d, t, in the above equation, the corresponding values of the coefficients τ, δ may be at once ascertained. τ and δ will of course vary, not irregularly, but continuously with the ratio

$$(d) \qquad \phi = \frac{c}{d} = \frac{B_e}{B_v}.$$

In order to express the law according to which this variation takes place, Gerber makes $x = \frac{c}{t}$, $y = \frac{d}{t}$, assumes the curve determined by x and y, having regard to the numerical values obtained by Wöhler, to be a parabola, and thus gets relations, by means of which δ, and therewith also

$$(e) \qquad \tau = \delta - \phi$$

may be determined. In general, if δ denote a constant depending on the nature of the material,

$$(f) \qquad \delta = \tfrac{1}{2}(\delta + \sqrt{\delta^2 + 4\phi^2 + 4\phi + 1}) \quad . \quad . \quad . \quad (4)$$

The values of δ can be previously calculated for regularly progressive values of ϕ and tabulated, as done by Gerber.

In all formulas B_e and B_v are to be substituted with their signs (tension positive, compression negative), so that ϕ becomes positive or negative according as B_e, B_v, have similar or opposite signs. If the straining force B_v due to the "live" load[1] act in the contrary sense to that due to the "dead" load, the total force may become $B_e + B_v = 0$, in which case $\phi = -1$. The values of B_r always have the same sign as $B_e + B_v$.

The practical application of the preceding method Gerber makes as follows:—

In order to take into account vibrations and impact, for the live load B_v is substituted n times that quantity, and then

$$\phi = \frac{B_e}{n\,B_v} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (5)$$

For this value of ϕ, δ is determined by (4), and there results

$$B_r = n\,\delta\,B_v \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (6)$$

$$F = \frac{B_r}{b_r} = \frac{n\,\delta\,B_v}{b_r} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (7)$$

Gerber chooses for iron $\delta = 1 \cdot 5$, $n = 1 \cdot 5$, for structures in which lightness is the principal requisite and small alterations of form are no disadvantage, $b_r = 2{,}400$, and for structures in which the greatest possible durability is required, $b_r = 1{,}600$ kilograms per square centimetre. Hence, for the latter

$$\phi = \frac{B_e}{1 \cdot 5\,B_v} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (8)$$

$$\delta = \tfrac{1}{4}\left(3 + \sqrt{16\,\phi^2 + 16\,\phi + 13}\right) \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (9)$$

$$F = \frac{1 \cdot 5\,\delta\,B_v}{1{,}600} \text{ square centimetres} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (10)$$

If the moving load may become positive as well as negative, ϕ, δ, F must be calculated for both limiting values B_v separately, and the sum of the numerical values $F = F_1 + F_2$ gives the actual sectional area.

The object now is to express the stress per unit of area allowed by Gerber.

---

[1] In the sequel the straining force due to the live load will be called simply the "live load," and that produced by the "dead" load the "fixed load," these terms referring to the forces applied to a single piece, and not to the *weight* on the structure of which the piece forms a part.

If a piece be subjected to tension only or compression only, then minimum $B \leqq B_o$.

$$\left.\begin{array}{l} \phi_1 = \dfrac{B_0}{n\,(\max B - B_0)}\ F_1 = \dfrac{n\,\delta_1}{b_r}\,(\max B - B_0)\ .\ \ .\ \ . \\[2ex] \phi_2 = \dfrac{B_0}{n\,(B_0 - \min B)}\ F_2 = \dfrac{n\,\delta_2}{b_r}\,(B_0 - \min B)\ .\ \ .\ \ . \end{array}\right\}(11)$$

From this follows the resultant maximum stress per unit of area

$$\left.\begin{array}{l} b = \dfrac{\max B}{F_1 + F_2} = \dfrac{1}{\delta_1\,(1 - \phi_0) + \delta_2\,(\phi_0 - \phi)}\,\dfrac{b_r}{n}\ \ .\ \ .\ \ . \\[2ex] \text{where with (1)} \\[1ex] \delta_1 \text{ from (4) corresponds to } \phi_1 = \dfrac{\phi_0}{n\,(1 - \phi_0)}\ \ .\ \ .\ \ . \\[2ex] \delta_2 \quad \text{,,} \qquad \text{,,} \qquad \text{,,}\ \phi_2 = -\dfrac{\phi_0}{n\,(\phi_0 - \phi)}\ .\ \ .\ \ . \end{array}\right\}(12)$$

If in a special case the fixed load coincides with one of the limiting loads, then either $\phi_0 = \phi$, $F_2 = 0$, or $\phi_0 = 1$, $F_1 = 0$, and from (12)

$$\left.\begin{array}{l} b = \dfrac{1}{\delta\,(1 - \phi)}\,\dfrac{b_r}{n}\ \ .\ \ .\ \ .\ \ .\ \ .\ \ .\ \ .\ \ . \\[2ex] \text{where with help of (1)} \\[1ex] \text{for } B_o = \min B,\ \delta \text{ from (4) corresponds to } \phi = \dfrac{\phi}{n\,(1 - \phi)} \\[2ex] \text{,,}\ \ = \max B,\ \delta\ \ \text{,,}\ \ (4) \qquad \text{,,} \qquad \text{,,}\ \phi = -\dfrac{1}{n\,(1 - \phi)} \end{array}\right\}(13)$$

If, on the other hand, a piece has to sustain alternations of tension and compression, there follows, according as the fixed load (numerical value B_0) has the same or the opposite sign as the higher limiting load (numerical value max B) with the upper or lower sign,

$$\phi_1 = \frac{\pm B_0}{n\,(\max B \mp B_0)}; \quad F_1 = \frac{n\,\delta_1}{b_r}\,(\max B \mp B_0)$$

$$\phi_2 = \frac{\mp B_0}{n\,(\max B^1 \pm B_0)}; \quad F_2 = \frac{n\,\delta_2}{b_r}\,(\max B^1 \pm B_0)$$

The greatest stress per unit of sectional area now amounts to

$$\left.\begin{array}{l} b = \dfrac{\max B}{F_1 + F_2} = \dfrac{1}{\delta_1\,(1 - \phi_0) + \delta_2\,(\phi_0 - \phi)}\cdot\dfrac{b_r}{n}\ \ .\ \ .\ \ . \\[2ex] \text{where with (2)} \\[1ex] \delta_1 \text{ from (4) corresponds to } \phi_1 = \dfrac{\phi_0}{n\,(1 - \phi_0)}.\ \ .\ \ .\ \ . \\[2ex] \delta_2\ \ \text{,,}\ \ (4) \qquad \text{,,} \qquad \text{,,}\ \phi_2 = \dfrac{\phi_0}{n\,(\phi - \phi_0)}.\ \ .\ \ .\ \ . \end{array}\right\}(14)$$

When in a special case the permanent load coincides with one of the limiting loads, either $\phi_0 = \phi$, $F_2 = 0$, or $\phi_0 = 1$, $F_1 = 0$, but by (14)

$$\left.\begin{array}{l} b = \dfrac{1}{\delta\,(1-\phi)}\dfrac{b}{n} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \\ \text{where by (2)} \\ \quad \text{for } B_0 = \max B^1, \delta \text{ from (4) corresponds to } \phi = \dfrac{\phi}{n\,(1-\phi)} \quad . \\ \quad \text{,, } B_0 = \max B, \ \delta \ \text{ ,, } \ (4) \qquad \text{,,} \qquad \text{,, } \phi = -\dfrac{1}{n\,(1-\phi)} \end{array}\right\} (15)$$

For iron structures of the greatest possible durability the values $n = 1 \cdot 5$, $b_r = 1{,}600$ would have to be substituted, in which case equation (4) merges into (9).

II.—SCHÄFFER'S MODIFICATION.

In Gerber's method, the way in which he proceeds in the case of the live load having different signs, provokes criticism, particularly with regard to such structural parts as are subjected alternately to tension and compression. The calculation is made as though there were two pieces, instead of taking into account the whole difference of stress at once in accordance with Wöhler's law. This drawback Professor Schäffer, of the Darmstadt Polytechnic, has avoided by a modification of Gerber's method. His first work on this subject appeared in 1874, and was supplemented by subsequent papers.[1]

Schäffer, in developing his theory, reversing Gerber's method, makes $x = \frac{d}{t}$, $y = \frac{c}{t}$; and recognising Gerber's conclusions, with regard to the relation between x and y, puts the latter into a somewhat different form by introducing

$$(a) \qquad \xi = \frac{d}{a},$$

a denoting the ultimate working strength. For this latter the following general expression results

$$a = \frac{-\delta\,\xi + \sqrt{\delta^2\,\xi^2 + (2-\xi)^2}}{(2-\xi)^2}\,2\,t \quad . \quad . \quad . \quad . \quad . \quad . \quad (16)$$

---

[1] Schäffer. "Bestimmung der zulässigen Beanspruckung für Eisenconstructionen." Zeitschrift für Bauwesen, 1874. Deutsche Bauzeitung, 1875, 1876. "Berücksichtigung der Zerknickungsgefahr." Deutsche Bauzeitung, 1877. Also von Willmann's essay on Schäffer's Method. Deutsche Bauzeitung, 1881.

where δ is the same constant, dependent on the nature of the material, used by Gerber.

The numerical values of the limiting straining forces resulting from the moving load only, producing tension solely or compression solely (min $B \leqq B_0$), are expressed as follows:—

$$B_1 = \max B - B_0 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (17)$$
$$B_2 = B_0 - \min B. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (18)$$

and for alternate tension and compression ($\max B' \leqq \max B$)

$$B_1 = \max B \mp B_0 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (19)$$
$$B_2 = \max B^1 \pm B_0 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (20)$$

In the latter case the upper or lower sign is to be used according as the fixed load (numerical value B_0) is of the same or opposite sense as the higher limiting load (numerical value max B).

As the difference of load for the whole section with tension or compression only is max B − min B, and for alternate tension and compression max B + max B^1, there follows with reference to (*a*) generally

$$(b) \qquad \xi = \frac{d}{a} = \frac{B_1 + B_2}{B_1 \pm B_0};$$

this value must of course always be positive.

If the ultimate working-strength *a* corresponding to the preceding value of ξ is to be determined, and the unit of area of the section subjected to this stress, the limit of destruction would just be attained without the action of impact and vibrations. In order to take the latter into account, Schäffer introduces all forces resulting from the moving load into the calculation multiplied with a factor *n*, even in determining ξ, and for further safety only the m^{th} part of the hypothetical working-strength corresponding to this value of ξ is used in the calculations. The determination of the sectional area then takes the following form:—

From the static calculation is determined

$$\xi = \frac{n\,(B_1 + B_2)}{n B_1 \pm B_0} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (21)$$

where B_0 has the upper or lower sign prefixed according as it corresponds to a load in a similar or an opposite sense to max B. Then equation (16) gives the hypothetical working strength *a*, and the sectional area becomes

$$F = \frac{m}{a}\,(n\,B_1 \pm B_0) \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (22)$$

Like Gerber, Schäffer adopts for iron the values $\delta = 1 \cdot 5$, $n = 1 \cdot 5$, and for permanent structures $\frac{t}{m} = b_r = 1{,}600$, so that

$$\xi = \frac{1 \cdot 5 (B_1 + B_2)}{1 \cdot 5\, B_1 \pm B_0} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (23)$$

$$\frac{a}{m} = \frac{-3\,\xi + \sqrt{13\,\xi^2 - 16\,\xi + 16}}{(2 - \xi)^2}\,1{,}600 \quad . \quad . \quad (24)$$

$$F = \frac{m}{a}\,(1 \cdot 5\, B_1 \pm B_0) \quad . \quad . \quad . \quad . \quad . \quad . \quad (25)$$

For $\xi = 2$, formula (24) gives $\frac{a}{m} = \frac{0}{0}$; the determination of the value of this expression leads to the result $\frac{a}{m} = 534$.

Schäffer's equations must give the same results as Gerber's when the fixed load coincides with one of the limiting loads, because in that case Gerber's division into two pieces does not enter into the calculation. Schäffer has also demonstrated how by his method liability to buckling may be taken into account.

With the help of equations (17)—(20), and (1) (2), there follows from (21) (22)

$$\xi = \frac{n\,(1 - \phi)}{n - (n - 1)\,\phi_0} \quad . \quad . \quad . \quad . \quad . \quad . \quad (26)$$

$$F = \frac{m}{a}\,(n \max B \mp (n - 1)\,B_0),$$

and hence the admissible stress per unit of area

$$b = \frac{\max B}{F} = \frac{1}{n - (n - 1)\,\phi_0}\,\frac{a}{m} \quad . \quad . \quad . \quad (27)$$

Here—in accordance with (26) $\frac{a}{m}$ by (16)—ϕ, and ϕ_0 are determined in the case of tension or compression only, from equation (1), and for alternate tension and compression by (2).

In particular for permanent iron structures

$$\xi = \frac{3\,(1 - \phi)}{3 - \phi_0} \quad . \quad . \quad . \quad . \quad . \quad . \quad (28)$$

$$b = \frac{2}{3 - \phi_0}\,\frac{a}{m} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (29)$$

III. MÜLLER'S SUGGESTION.

Mr. Müller, a Viennese engineer, published in 1873 an essay on the "Determination of Dimensions." He starts from the assumption that every stress exceeding the limit of elasticity, that is, accompanied by a permanent alteration of form (set), must, if repeated sufficiently often, produce fracture. The primitive strength u, as the smallest stress in one sense which can practically cause fracture, is identical with the usual limit of elasticity. With smaller differences of stress, or larger values of $\frac{c}{d}$—notation as under I.—fracture can only be brought about by the greater working stress a; hence there exist, according to the ratio $\frac{c}{d}$, an infinite number of limits of elasticity, varying from u to the statical breaking strength t.

If all values of c assumed by Wöhler be represented as abscisses (tension positive, compression negative), and the corresponding values of a, determined experimentally, as ordinates, a curve is obtained which Müller prolongs until it intersects the c-axis, arriving by means of analogies, which, however, are not precisely defined, at the value of the primitive strength for compression, and thus completing Wöhler's data. From this curve it would be possible for every given value

$$(a) \qquad \phi = \frac{c}{d} = \frac{B_c}{B_a},$$

to determine the working strength a, and allowing a suitable factor of safety the admissible stress b.

Müller[1] considers a factor of safety of 3 as suitable, but intends when using the value $b = \frac{a}{3}$ to take into account the influence of temperature and corrosion separately in previously determining a.

The influence of a rise of temperature is taken as equivalent to the stress which would produce the same extension.

It is stated that the influences of temperature and load are fortunately not altogether cumulative, but that each takes a separate part in the wear and tear, and "this circumstance clearly tends to reduce the absolute maximum stress with a large perma-

---

[1] G. Müller, "Zulässige Inanspruchnahme des Schmiedeisens bei Brücken-constructionen." Zeitschrift des Östreich. Ing. u. Arch. Vereins, 1873.

nent load, because when stresses due to other causes come into action, the danger of reaching the absolute limit of fracture is increased."

In accordance with this the ratio $\beta = \frac{a}{u}$ is modified in a way not quite clear, and determined for a series of values of ϕ from the completed curve representing Wöhler's results, whence

$$(b) \qquad b = \tfrac{1}{3}\,\beta\,u.$$

Here Müller makes $u = 1{,}600$, and calculates accordingly two tables of admissible stresses, one for tension alone, and one for alternations of tension and compression.

It is easy to perceive that in the preceding method untenable assumptions are included. It is by no means the case that everything tends to show that a single increase of temperature "exerts exactly the same influence on a member of a bridge as a single application of a load"; but on the contrary experience hitherto has been opposed to this. It has been observed that at temperatures of 100° to 200° Centigrade equal and greater loads are carried than at ordinary temperatures, although both influences, which are supposed to act in the same sense, are cumulative. Neither can the choice of a primitive strength for compression, which exceeds that for tension, in the total absence of experiments in this direction, be approved.

IV. WINKLER'S METHOD.

In the year 1877 Dr. Winkler, Professor at the Polytechnic of Berlin, published a method of determining dimensions,[1] which may be briefly described as follows.

If for given values $\pm a^1$ of the numerically smaller limiting stress per unit of area as abscisses, the corresponding values of the working strength a be plotted as ordinates in accordance with Wöhler's experiments, Winkler finds that when a represents a tensile stress, the ends of the latter are grouped sufficiently closely about a straight line to justify the equation.

$$(a) \qquad a = u \pm a\,a^1.$$

This formula is temporarily assumed to be applicable for compres-

---

[1] E. Winkler, "Wahl der zulässigen Beanspruchung für Eisenconstructionen. Zeitschr. d. Östr. Ing. u. Arch. Vereins, 1877. *Vide* also Herzmannsky, "Ueber das Winklersche Verfahren." Wochenschrift d. Östr. Ing. u. Arch. Vereins, 1877.

sion also, so that generally speaking the upper or lower sign is to be taken before a^1, according as the latter value represents a stress in a similar or opposite sense to a.

With a static load $a = a^1 = t$, therefore

$$(b) \qquad t = u + a\,t \qquad u = (1 - a)\,t.$$

$$(c) \qquad a = (1 - a)\,t \pm a\,a^1.$$

Assuming a factor of safety m, the admissible stress is $\frac{a}{m}$; hence if temporarily B_g represent the numerical value of the numerically smaller limiting load,

$$(d) \qquad F = \frac{m \cdot \max B}{a} = \frac{m \cdot B_g}{a^1}.$$

From this equation there follows, with reference to the preceding,

$$(e) \qquad \max B = \frac{1 - a}{m} F\,t \pm a\,B_g,$$

and if the static stress admissible with a factor of safety m be again denoted by $\frac{t}{m} = b_r$, then

$$(f) \qquad F = \frac{\max B \mp a \cdot B_g}{(1 - a)\,b_r},$$

here the upper or lower sign must be taken according as the smaller limiting load is of the same kind as (numerical value $B_g = \min B$) or opposite to (numerical value $B_g = \max B^1$) the greater. By substitution of the values min B and max B from (17)—(20) there follows generally

$$(g) \qquad F = \pm \frac{B_0}{b_r} + \frac{B_1}{(1 - a)\,b_r} + \frac{a\,B_2}{(1 - a)\,b_r}.$$

In order to take account of impact and vibrations, Winkler introduces the live load multiplied by n into the calculation, whence

$$F = \pm \frac{B_0}{b_r} + \left[\frac{B_1}{(1 - a)\,b_r} + \frac{a\,B_2}{(1 - a)\,b_r}\right] n \quad . \quad . \quad . \quad (30)$$

When max B denotes tension, Winkler makes $b_r = 1{,}400$, $a = 45$, and, after rounding off the results, obtains for structures not subject to impact and with $n = 1$,

$$F = \pm \frac{B_0}{1{,}400} + \frac{B_1}{770} + \frac{B_2}{1{,}700} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (31)$$

for railway bridges, with $n = 1 \cdot 3$,

$$F = \pm \frac{B_0}{1{,}400} + \frac{B_1}{590} + \frac{B_2}{1{,}300} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (32)$$

for road bridges, with $n = 1 \cdot 2$,

$$F = \pm \frac{B_0}{1{,}400} + \frac{B_1}{640} + \frac{B_2}{1{,}400} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (33)$$

When, however, max B denotes compression, the values should be $b_r = 1{,}200$, $a = 0 \cdot 40$, so that then, for structures not subject to impact with $n = 1$,

$$F = \pm \frac{B_0}{1{,}200} + \frac{B_1}{720} + \frac{B_2}{1{,}800} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (34)$$

for railway bridges, with $n = 1 \cdot 3$,

$$F = \pm \frac{B_0}{1{,}200} + \frac{B_1}{550} + \frac{B_2}{1{,}380} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (35)$$

for road bridges, with $n = 1 \cdot 2$,

$$F = \pm \frac{B_0}{1{,}200} + \frac{B_1}{600} + \frac{B_2}{1{,}500} \cdot \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (36)$$

In all formulas B_0 has the + or − sign, according as B_0 represents a load in the same sense as, or opposite to, max B. Hence for tension only or compression only the + sign must be used.

Winkler recommends that formulas (31)—(33) should be used even when max B denotes compression, provided that, as in the case of tension, under F the nett sectional area (after deducting the rivet-holes) be understood.

If in formula (30) B_1 and B_2 are expressed by (17)—(20) in terms of the limiting loads, there follows with reference to the expressions (1) and (2),

$$b = \frac{\max B}{F} = \frac{(1 - a)\, b_r}{n\,(1 - a\,\phi) - (n - 1)\,(1 - a)\,\phi_0} \quad . \quad . \quad (37)$$

For railway bridges in particular there results with $n = 1 \cdot 3$, where max B denotes tension and $b_r = 1{,}400$, $a = 0 \cdot 45$,

$$b = \frac{770}{1 \cdot 3 - 0 \cdot 585\,\phi - 0 \cdot 165\,\phi_0} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (38)$$

and if max B denote compression, and $b_r = 1{,}200$, $a = 0 \cdot 40$,

$$b = \frac{720}{1 \cdot 3 - 0 \cdot 52\,\phi - 0 \cdot 18\,\phi_0} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (39)$$

Some objections to the preceding formulas cannot be overlooked.

For alternate tensile and compressive loads of equal magnitude, that is for $\phi = -1$, either the tensile or compressive load may be regarded as max B, and then both formulas (32) and (35), or (38) and (39), should yield equal values. Instead of this, however, for $\phi = -1$ and the subjoined values of B_0 and ϕ_0 the following results are obtained:—

B_0	ϕ_0	by (38)	ϕ_0	by (39)
Compression .	-1	376	$+1$	439
" .	$-\frac{1}{2}$	391	$+\frac{1}{2}$	416
0	0	408	0	396
Tension . .	$+\frac{1}{2}$	427	$-\frac{1}{2}$	377
" . .	$+1$	448	-1	360

From this it appears that differences up to 17·5 per cent. of the mean values from both formulæ may occur.

That Winkler should take the admissible stress for compression without liability to buckling as smaller than for tension (1,200 as against 1,400 for b_r) is remarkable, and altogether the expression adopted for a does not appear sufficiently warranted by Wöhler's results. In order to arrive at such expressions, experiments made with the same material and under the same conditions only should be utilised, because only in this case does the effect of variable loads undisturbed by other influences show itself.

For unhardened Krupp spring-steel with $u = 500$ centners primitive strength, and $t = 1{,}100$ centners at least per square inch Rhenish, with which the most complete experiments were made, the following are the results obtained by different methods with initial stresses:—

	$a^1 =$ 0	250	400	600	1,100
Wöhler's experiments[1]	$a =$ 500	700	800	900	(1,100)
Launhardt's formula	$a =$ 500	711	800	900	1,100
Winkler's formula	$a =$ 500	612	680	770	995

V. FORMULAS OF CAIN, SMITH, AND SEEFEHLNER.

In order to make the well known empiric formulæ for liability to buckling agree with some older proposals of a committee of the American Society of Civil Engineers, Professor Cain of Char-

---

[1] Wöhler, "Die Festigkeitsversuche mit Eisen und Stahl," Berlin, 1870, p. 7.

lotte, North Carolina, adopted, as representing the admissible static stress of iron bars subjected to compression, the expression

$$(a) \qquad b_r = \frac{1}{4 + \frac{1}{10}\left(\frac{l}{d} - 15\right)} \frac{t}{1 - \delta\left(\frac{l}{r}\right)^2},$$

where l denotes the length of bar, $r^2 = \frac{I}{F}$, the square of the least radius of gyration of the section, d the diameter of the bar at right angles to the axis for θ, t the crushing stress, δ a constant, having for bars held fast at both ends, or supported on flat surfaces, the value 1 : 36000, for bars attached by pins at both ends 1 : 18000, and for bars held fast at one end and attached by a pin at the other 1 : 24000.

In recognising Wöhler's experiments Cain has regard principally to lattice girders.[1] He assumes that the effect of impact diminishes with increasing weight of the members of a structure. That the weight of the web-members increases tolerably uniformly from the middle towards the ends of the girders, equally so the ratio ϕ of the limiting loads on the members. Consequently it may be assumed with sufficient accuracy that the effect of impact increases proportionally with ϕ. Hence if for iron, by Launhardt and Weyrauch's formula $b = c\left(1 + \frac{\phi}{2}\right)$, then the empirical value to be used expressed in kilograms per square centimetre would be

For rods in tension $b = 525\,(1 + \phi)$ . . . . . (40)

For members in compression

$$b = \frac{1}{4 + \frac{1}{10}\frac{l}{d}} \frac{2{,}700}{1 + \delta\left(\frac{l}{r}\right)^2}(1 + \phi) \quad . \; . \; . \; . \quad (41)$$

for alternate tension and compression Cain simply makes $\phi = 0$.

Mr. Seefehlner, engineer of Budapest, in applying Launhardt and Weyrauch's method, wished to take separately into account the effect of impact.[2] In the first instance, for the ultimate working strength of iron, he sets

$$(b) \qquad a = u\left(1 + \frac{\phi}{2}\right) = \frac{2\,t}{3}\left(1 + \frac{\phi}{2}\right).$$

---

[1] Cain in Van Nostrand's Eclectic Engineering Magazine, 1877.

[2] Seefehlner in Zeitschrift des Östreichischen Ing. u. Arch. Vereins, 1878.

"If it is desirable to include in the factor of safety m the influence of the impact of the moving load, it may be considered that the influence of the latter is greater for small bridges than for large spans, Wöhler's experiments also show that as the difference in the limiting stresses decreases the working strength increases, hence may be written

$$m = \mathrm{A} - \mathrm{B}\,\phi,\text{"}$$

and therefore the admissible stress per unit of area in general for iron

$$b = \frac{a}{m} = \frac{2 + \phi}{\mathrm{A} - \mathrm{B}\,\phi}\frac{t}{3} \quad . \quad . \quad . \quad . \quad (42)$$

here Seefehlner chooses the following values, $t = 3{,}600$, $\mathrm{A} = 4$, $\mathrm{B} = 1 \cdot 6$, consequently

$$b = \frac{2 + \phi}{4 - 1 \cdot 6\,\phi}\,1{,}200 \quad . \quad . \quad . \quad (43)$$

$$\mathrm{F} = \frac{\max \mathrm{B}}{b} = \frac{4 - 1 \cdot 6\,\phi}{2 + \phi}\,\frac{\max \mathrm{B}}{1{,}200} \quad . \quad . \quad (44)$$

The imperfection of the Launhardt and Weyrauch method induced Professor Smith of Birmingham to construct a new formula based upon Wöhler's experiments.[1] He makes the difference of stress

$$(c) \qquad d = \frac{4\,(t_d + e)\,(t_s - e)\,u_s}{(2\,t_d + u_s)\,(2\,t_s - u_s)} = \frac{4\,(t_d + e)\,(t_s - e)\,u_d}{(2\,t_d - u_d)\,(2\,t_s + u_d)},$$

where t_s, t_d, are the statical breaking strengths, and u_s, u_d, the primitive strengths for tension and compression respectively, these all being numerical values without + or − signs; e, on the other hand, is the arithmetical mean of the limiting stresses per unit of area, where tension is to be taken as positive and compression negative. As for alternations of equal tensile and compressive loads d is equal to twice the vibration strength s, there follows from (c)

$$(d) \qquad s = \frac{2\,t_s\,t_d\,u_s}{(2\,t_d + u_s)\,(2\,t_s - u_s)} = \frac{2\,t_d\,t_s\,u_d}{(2\,t_d - u_d)\,(2\,t_s + u_d)},$$

and hence generally

$$(e) \qquad d = \frac{(t_d + e)\,(t_s - e)}{t_d\,t_s}\,2\,s.$$

---

[1] Smith in "Engineering," 1880.

If temporarily, as the values u_d, t_d, are not known, it is assumed that $u_d = u_s = u$, $t_d = t_s = t$, there follows:—

$$(f) \qquad d = \frac{4\,u\,(t^2 - e^2)}{4\,t^2 - u^2} = \frac{t^2 - e^2}{t^2}\,2\,s,$$

As to the practical application of these formulas no particulars are given.

With regard to a method of taking into account the effect of impact as desired by Gerber, Seefehlner, Schäffer and Winkler, a difference of opinion may exist on the following grounds: (1) the influence of loads which do not gradually increase is already included in Wöhler's results; (2) the effects of impact are not greatest on those members for which the live load B_v is a maximum, while for those parts which, like the rail-bearers, are directly exposed to impact, the admissible stress is in any case taken as smaller than for other portions; (3) the influence of differences of stress on bridges must be less than in the case of Wöhler's experiments, in which all the straining actions followed each other very quickly, and this, even when Wöhler's results are taken into account, may be noticed by dividing the influence of the moving load by n_1; (4) according to Fairbairn, Vicat, Thurston, among others, a continuance of the stress is unfavourable, and this might lead to the practice of introducing the fixed load B_e into the calculation multiplied with the factor n_2 as compared with the live load B_v; (5) by a separate treatment of the fixed and live load, not only the determination of dimensions but also the static calculation becomes more complicated, because then B_e and the two limiting values B_v have to be reckoned separately, while otherwise only the limiting loads are used. Let it be now assumed that the influence of impact alone makes it desirable to multiply B_v by n, then by (3) and (4) there would have to be taken into calculation the quantity

$$(g) \qquad B = n_2\,B_e + \frac{n}{n_1}\,B_v = \frac{n}{n_1}\left(\frac{n_1\,n_2}{n}\,B_e + B_v\right).$$

By introducing simply B_e and B_v into the calculation, and taking account of the effects of impact by a general factor of safety, there results $\frac{n_1\,n_2}{n} = 1$, which, in the absence of further data, appears to be generally most suitable.

If, however, the influence of impact is to be accounted for in the way assumed by Gerber, Schäffer, and Winkler, namely, by introducing the limiting forces B_v B_v^1, resulting from the moving load, into the calculation multiplied with the factor *n* (tension positive, compression negative), this can in every case be done. As hypothetical limiting loads the expressions

$$(h) \qquad B = B_e + n\,B_v.$$

$$(i) \qquad B = B_e + n\,B_v^1.$$

are taken, the ratio of the numerically smaller to the numerically greater of these values is denoted by ϕ, and then (even when liability to buckling is considered) the method of proceeding is exactly as though B and B were the actual limiting loads.[1] Of course, as the influence of impact is already accounted for, a more favourable factor of safety may be chosen.

Krohn's[2] method deviates somewhat from this, as from the commencement he reckons with 1·5 time the moving load, but then allows the value

$$(k) \qquad b = 1{,}000\left(1 + \frac{\phi}{2}\right).$$

In the present state of knowledge on the subject, and with regard to the matter generally, excessive refinements appear to the Author unsuitable.

VI. Ritter's Hypothesis.

In a Paper of the year 1877,[3] Mr. Fr. Ritter, an engineer of Vienna, aims at placing the facts relative to the strength of materials proved by Wöhler's experiments upon a deeper foundation.

According to Ritter, the assumption is a plausible one, that the destructiveness of repeated stresses is inversely proportional to their distance from the statical breaking strength *t*. The resulting destructiveness of every stress varying between the limits *a* and ± a^1 may not exceed a certain value for a given material. "This value is independent of the relative position of the vibration as regards *o* and *t*, so that when the position changes, and, according to the preceding, each individual oscillation becomes more or less

---

[1] For further information, *vide* Crugnola -Weyrauch, "Stabilità delle costruzioni in ferro ed in acciao," Turin 1879, sec. 36; and Zeitschr.. d. Östr. Ing. u. Arch. Vereins, 1878.

[2] Krohn, "Resultate aus der Theorie des Brückenbaues," Aachen, 1879.

[3] Fr. Ritter, "Die Festigkeitsverhaltnisse nach den Wöhlerschen Versuchen," Wochenschrift d. Östr. Ing. u. Arch. Vereins, 1877.

destructive, not the sum of the individual destructiveness is altered, but on the other hand the admissible limits of the resulting vibration become narrower or wider." Ritter claims to express this hypothesis by the formula

$$(a) \qquad \int_{\pm a^1}^{a} \frac{t}{t-a}\, d\, a = \text{const},$$

whence when k is a constant for tension or compression only

$$(b) \qquad \frac{t-a^1}{t-a} = k,$$

and for alternate tension and compression on account of the expression

$$\int_{-a^1}^{a} = \int_{o}^{a} + \int_{o}^{a^1}$$

$$(c) \qquad \frac{t}{t-a} \cdot \frac{t}{t-a^1} = k.$$

For a factor of safety m, the admissible stress per unit of area $b = \frac{a}{m}$ and $a = m\,b$. As by (1) for a piece subjected always to tension or always to compression $a^1 = \phi a$, there follows from (b)

$$(d) \qquad \frac{t - m\,\phi\,b}{t - m\,b} = k$$

$$b = \frac{k-1}{k-\phi}\,\frac{t}{m} \quad . \quad . \quad . \quad . \quad . \quad . \qquad (45)$$

As further, with alternate tension and compression, by (2), $a' = -\phi a$ there results from (c)

$$(e) \qquad \frac{t}{t - m\,b}\,\frac{t}{t + m\,\phi\,b} = k$$

$$b = \left[-\frac{1-\phi}{2\,\phi} - \sqrt{\left(\frac{1-\phi}{2\,\phi}\right)^2 + \frac{k-1}{k\,\phi}} \right] \frac{t}{m} \quad . \quad . \qquad (46)$$

For iron in particular Ritter makes $k = 2$, $m = 3$, $t = 3{,}600$, whence for tension only or compression only

$$b = \frac{1{,}200}{2-\phi} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (47)$$

$$\text{F} = \frac{\max \text{B}}{b} = \frac{2-\phi}{1{,}200} \max \text{B} \quad . \quad . \qquad (48)$$

and for alternate tension and compression

$$b = \frac{\phi - 1 + \sqrt{1 + \phi^2}}{\phi} 600 \quad . \quad . \quad . \quad (49)$$

$$F = \frac{\phi}{\phi - 1 + \sqrt{1 + \phi^2}} \cdot \frac{\max B}{600} \quad . \quad . \quad . \quad (50)$$

For $\phi = 0$ the last equation gives $\frac{0}{0}$, then however (43) is applicable and gives $b = 600$.

VII. LIPPOLD'S METHOD.

Whereas the methods hitherto mentioned leave it unexplained why by variable loads fracture is produced more easily than by a static load, engineer Lippold of Wiesbaden gives Wöhler's laws the following form[1]: "In order to fracture a piece, a certain amount of work is necessary, and this may be accumulated in the material at once, as well as by repeated intermittent loads. These loads, however, must recur instantaneously, or within so short a time that vibrations are produced." It follows that only through the occurrence of vibrations can extension, and with it stress and work, equal to what will overcome the statical breaking strength, be produced by a load lower than the statical breaking load.

Importance is attached to repetitions of the load only in so far as by these the limit of elasticity can be brought up nearly to the limit of fracture, in which case the relations depending on the proportionality of stress and extension are approximately applicable up to the point of fracture. In conformity with the preceding interpretation of Wöhler's law, Lippold, in determining the admissible stress, starts from the following rule: "On no member of a structure shall more work be performed by the fixed and live load than would be done by a weight slowly increasing from nothing up to the amount of the static load considered admissible." If for a piece subjected to tension or compression, l denote the original length, F the sectional area, x the momentary extension or compression, E the modulus of elasticity, the piece tries to regain its original condition with a force

$$(a) \qquad X = \frac{F\,E}{l} x = \frac{x}{a}.$$

---

[1] H. Lippold, "Die Inanspruchnahme von Eisen und Stahl mit Rucksicht auf bewegte Last." Organ f. d. Fortschritte des Eisenbahnwesens, 1879.

A further extension by the amount dx may be produced by the work $X d x$, and the extension of the piece from 0 to x requires the work

$$(b) \qquad \int_0^x X\, d\, x = \frac{x^2}{2\, a} = \frac{X\, x}{2} = \frac{a\, X^2}{2}.$$

If the load increases gradually from nil up to the admissible static load $R = F\, b_r$ the work done is spent only in overcoming the elastic force, and the work accumulated in the piece is by (b)

$$(c) \qquad A = \frac{a\, R^2}{2} = \frac{a}{2} (F\, b_r)^2.$$

According to the above rule more work than this must not be put into the piece (in other words, the potential energy corresponding to the elastic force must not exceed this value).

Let there be equilibrium at a given moment between the load P acting on the piece and the elastic force. Then the piece[1] contains the work $\frac{a\, P^2}{2}$, the alteration of length according to (a) is $\lambda = a\, P$. Now let a new load Q, acting in the same sense as P, be suddenly added; then the load exceeds the resistance, a part of the work of Q is transformed into kinetic energy (*vis viva*), which, however, is gradually expended in overcoming the increasing resistance. At that moment, when no more kinetic energy remains and the greatest alteration of length $\lambda + \lambda'$ has been attained, the work done by the loads—apart from other applications (heat)—must be equal to the work of the elastic force overcome. The latter work by (b) amounts to $\frac{(\lambda' + \lambda)^2}{2\, a}$, so that

$$(d) \qquad \frac{a\, P^2}{2} + (P + Q)\, \lambda' = \frac{(\lambda' + \lambda)^2}{2\, a}$$

and hence with reference to $\lambda = a\, P$

$$(e) \qquad \lambda' = 2\, a\, Q.$$

The new alteration of length is accordingly independent of that previously existing, and twice as great as with a gradually applied

---

[1] P may also have been suddenly applied. In this case the alteration of length would, in the first instance, have been greater than λ, vibrations about the position of equilibrium λ take place; but when it is attained only the energy $\frac{a\, P^2}{2}$ remains behind in the piece, the rest has been principally converted into heat.

load. If now the total work spent on the piece is not to exceed the value (*c*), there follows from (*d*)

$$(f) \qquad \frac{a\,P^2}{2} + (P + Q)\,2\,a\,Q = \frac{a}{2}\,(F\,b_r)^2,$$

and hence the requisite sectional area

$$(f) \qquad F = \frac{P + 2\,Q}{b_r}.$$

Suppose that equilibrium exists again between a load P acting on the piece and the elastic force. Momentary alteration of length λ, work accumulated in piece $\frac{a\,P^2}{2}$. Suddenly a load $Q > P$, acting in the opposite sense to the latter, is applied, so that the alteration of length λ ceases, and one in the opposite sense $\lambda' - \lambda$ is produced. In neutralising λ the force $Q - P$ and the elastic force act in the same sense, resistance only beginning with the alteration of length $\lambda' - \lambda$ in the opposite direction; and neglecting other applications of the work

$$(g) \qquad \frac{a\,P^2}{2} + (Q - P)\,\lambda' = \frac{(\lambda' - \lambda)^2}{2\,a}.$$

(As the original length or zero of the elastic force is approached, the potential energy $\frac{a\,P^2}{2}$ is converted into kinetic, and this, as well as the additional work $(Q - P)\,\lambda'$ coming from an external source, are, when the zero is passed, again transformed into potential energy, the value of which at the distance $\lambda' - \lambda$ from the zero is $\frac{(\lambda' - \lambda)^2}{2\,a}$).

As in the first case, equation (*g*), taken in conjunction with $\lambda = a\,P$, gives

$$(e) \qquad \lambda' = 2\,a\,Q.$$

Here also the total alteration of length resulting from Q is independent of that previously existing, and twice as great as with a gradually applied load.

If, again, the work spent on the piece is not to exceed the value (*c*), then follows with regard to (*e*)

$$(h) \qquad \frac{a\,P^2}{2} + (Q - P)\,2\,a\,Q = \frac{a}{2}\,(F\,b_r)^2,$$

and hence the requisite sectional area

$$(i) \qquad F = \frac{2Q - P}{b_r}.$$

If, now, a member of a bridge has to be calculated, equilibrium may be supposed established, not only after the action of the fixed load, but after every straining action. If, then, a new load be suddenly applied, the preceding formulas are applicable. In calculating the sectional area, however, that case must be selected which gives the greatest value of F. Accordingly we obtain the necessary section and the admissible stress per unit of area:

For tension or compression only from (*f*), $P + Q = \max B$, $P = \min B$.

$$F = \frac{2 \max B - \min B}{b_r} \quad . \quad . \quad . \quad . \quad (51)$$

for alternate tension and compression from (*i*) where $Q - P = \max B$, $P = \max B'$

$$F = \frac{2 \max B + \max B'}{b_r} \quad . \quad . \quad . \quad . \quad (52)$$

and with regard to (1) and (2) in both cases

$$b = \frac{\max B}{F} = \frac{b_r}{2 - \phi} \quad . \quad . \quad . \quad . \quad (53)$$

in these formulas Lippold substitutes for wrought iron the value $b_r =$ 1,300, for unhardened cast steel $b_r = 2{,}400$ per square centimetre.

Formula (53) agrees for tension only or compression only with Ritter's (45), in which $K = 2$.

In Lippold's very remarkable work views are put forward and developed which, previous to Wöhler's labours, had many supporters,[1] and the correctness of which cannot be disputed. The question is, however, whether this view of the subject is not one-sided, whether the destruction of the material is solely, or even mainly, due to the causes indicated.

If that be the case, then, Wöhler's law and results would lose much of their significance, as they are unnecessary for the development of the formulas and numerical values given.

It is certainly not creditable that Lippold's work has remained almost unnoticed in Germany, especially on the part of those to whom the management of technical experiments is entrusted. The

---

[1] *Vide* Von Kaven's "Collectaneen," Zeitschr. des hannöv. Arch. u. Ing. Vereins of January, 1868.

experiments hitherto made leave no doubt as to the general law, but only as to the causes of it.

Although every one is at present free to choose whether he will accept a theoretical explanation or simply remain contented with the experimental results, it is most certain from Lippold's theory, that the use of a constant coefficient of strength for determining the dimensions of engineering structures is totally indefensible.

VIII.—CLERICETTI'S EQUATIONS.

The latest Paper on the application of Wöhler's experiments is by Professor Clericetti of the Technical Institute of Milan.[1] Clericetti arrives in the first instance at the same conclusion as Lippold, but subsequently investigates separately the influence of repetitions of the load.

A sudden application of a load to a piece causes temporarily twice the extension (VII.), and therefore twice the increase of stress due to the same load gradually applied. This applies in the first instance to stresses within the limit of elasticity, as, however, the latter, by being exceeded, may be brought up nearly to the limit of fracture, Clericetti considers that a sudden load should be treated even until fracture occurs, as a quiescent load of twice the magnitude. As a further test, for a piece having a unit of sectional area, and to which the stress due to fixed load c, the difference of stress in the same sense d, and a statical breaking strength, t, apply, the formula is given

$$(a) \qquad c + 2d = t$$

The stress at the moment of fracture by the ordinary method would be

$$(b) \qquad c + d = a$$

so that the ultimate working strength for a load quiescent only as to one portion c is

$$(c) \qquad a = \frac{c + t}{2}.$$

This formula agrees very well with Wöhler's results, as the following comparison shows:—

For t = 1,100 (compare IV.) and

	c	=	0	250	400	600	1,100
by Wöhler	a	=	500	700	800	900	1,100
„ formula (c)	a	=	550	675	750	850	1,100

---

[1] C. Clericetti, "Sulla determinazione dei coëfficienti di sferzo specifico dietro le esperienze di Wöhler," Il Politecnico, 1881.

It must, however, be remembered that in these experiments fracture occurred, not after a few applications of load, but only after 40,000,000 and more; while Kirkaldy found that with a sudden application the breaking weight varied for different kinds of iron from 75·2 to 90·4 per cent., on an average 81 per cent.[1] of that required with a gradual application.

Lippold ascribes this deviation of 50 per cent. to the circumstance that the loads had not sufficient time to produce the full extension and with it the full stress, the energy not used in overcoming the resistance of the piece being converted into heat.

Formula (a) is distinguished from that with which Gerber starts (I (a))

$$c + \tau d = t$$

only by the choice of the coefficient τ, which Gerber determines from experimental results. In fixing the admissible stress Clericetti proceeds as follows:—

Repeated Loads in one sense.—Let B_0, B_1, denote respectively the fixed and live loads $\max B = B_0 + B_1$, m a factor of safety, u the primitive strength, then Clericetti writes

$$F = \frac{B_0 + 2\,B_1}{u} m = \frac{2 \max B - B_0}{u} m \quad . \quad . \quad . \quad . \quad . \quad . \quad (54)$$

According to the usual method the stress per unit of area, and with regard to (1) is

$$b = \frac{\max B}{F} = \frac{B_0 + 2\,B_1}{B_0 + B_1} \frac{u}{m} = \frac{1}{2 - \phi_0} \frac{u}{m} \quad . \quad . \quad . \quad (55)$$

If the load is in the same sense on the whole, moving loads in opposite senses may still occur. Clericetti would apply the preceding formulas to this case also.

Particulars are not given; but it appears that then ϕ should be substituted for ϕ_0 (which is correct when $B_0 = \min B$), whence generally

$$b = \frac{1}{2 - \phi} \frac{u}{m} \quad . \quad . \quad . \quad . \quad . \quad (56)$$

The formula differs from Lippold's (53), in so far as in the latter $\frac{t}{m} = b_r$ takes the place of $\frac{u}{m}$. With a suitable choice of constants, both these formulas, as well as Ritter's (45) lead to equal values of b.

---

[1] Von Kaven's "Collectaneen," Zeitschrift des hannöv. Arch. u. Ing. Vereins, 1868.

Repeated Loads in opposite senses.—Let B_0, B_1, B_2 denote the numerical values of the fixed load and the extreme live load. On the assumption of the sudden application of the live load the actual stress varies according to Clericetti—when B_0 B_1 act in the same sense—between

$$\pm \frac{B_0 + 2\,B_1}{F} \text{ and } \mp \frac{2\,B_2}{F},$$

when B_0, B_2 denote loads in the same sense

$$\pm \frac{B_0 + 2\,B_2}{F} \text{ and } \mp \frac{2\,B_1}{F};$$

hence in both cases the difference of stress would be

$$(d) \qquad d = \frac{B_0 + 2\,B_1 + 2\,B_2}{F};$$

and s being the vibration strength, Clericetti makes the sectional area

$$F = \frac{B_0 + 2\,B_1 + 2\,B_2}{2\,s}\,m \quad . \quad . \quad . \quad . \quad (57)$$

where $m\,d = 2\,s$ has been introduced. As by (19) and (20) $B_1 + B_2 = \max B + \max B_1$ there follows also

$$F = \frac{\max B + \max B_1 + \frac{1}{2}\,B_0}{s}\,m \quad . \quad . \quad (58)$$

and having regard to (2) the stress per unit of area is

$$b = \frac{\max B}{B_1 + B_2 + \frac{1}{2}\,B_0}\,\frac{s}{m} = \frac{1}{1 - \phi \pm \frac{1}{2}\,\phi_0}\,\frac{s}{m} \quad . \quad . \quad . \quad (59)$$

Herein the upper or lower sign is applicable to ϕ_0, according as the fixed load is of the same or opposite kind as the upper limiting load, that is, always that sign which will make the quantity $\pm \frac{\phi_0}{2}$ positive.

Numerical Values.—Clericetti takes the following values of t, u, s, as averages from Wöhler's experiments, for iron 3,800, 2,450, 1,385, for steel 6,800, 3,400, 2,000, adopts the factor of safety $m = 2$, and thus obtains, in the case of iron, for tension only or compression only

$$F = \frac{B_0 + 2\,B_1}{1{,}250} \quad . \quad . \quad . \quad . \quad . \quad . \quad (60)$$

$$b = 1{,}250\,\frac{B_0 + B_1}{B_0 + 2\,B_1} = \frac{1{,}250}{2 - \phi_0} \quad . \quad . \quad . \quad (61)$$

For alternate tension and compression

$$F = \frac{B_1 + B_2 + \frac{1}{2} B_0}{650} \quad . \quad . \quad . \quad . \quad (62)$$

$$b = 650 \frac{\max B}{\frac{1}{2} B_0 + B_1 + B_2} = \frac{650}{1 - \phi \pm \frac{1}{2} \phi_0} \quad . \quad . \quad . \quad (63)$$

According to the principles of the preceding method itself, for alternate tension and compression, the actual stress should not be assumed as above, but as varying between

$$\pm \frac{B_0 + 2 B_1}{F} \text{ and } \pm \frac{B_0 - 2 B_2}{F},$$

and

$$\pm \frac{B_0 + 2 B_2}{F} \text{ and } \pm \frac{B_0 - 2 B_1}{F},$$

respectively. (*Vide* also X).

Hence would follow, in a way similar to the preceding,

(*e*)

$$d = \frac{B_1 + B_2}{F} 2$$
$$b = \frac{1}{1 - \phi} \frac{s}{m} \quad . \quad . \quad . \quad . \quad . \quad . \quad (64)$$

Formulas (56) (64) give, then, for iron, with tension or compression only,

$$b = \frac{1{,}250}{2 - \phi}; \quad . \quad . \quad . \quad . \quad . \quad . \quad (65)$$

and with alternations of tension and compression, on account of $s = 1{,}385$ and $m = 2$,

$$b = \frac{700}{1 - \phi} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (66)$$

For $\phi = 0$, $\phi_0 = 0$, equal values of b should result from the formulas for tension or compression only, and from those for alternate tension and compression. The way in which u and s are introduced into the above formulas can, however, scarcely be justified, and, after what has been said of a at the commencement, is surprising.

Equation (64), with a suitable choice of constants, gives the same results as a rule followed by American engineers previous to Wöhler's experiments, according to which for alternate tension and compression

$$F = \frac{\max B + \max B_1}{c} \quad . \quad . \quad . \quad . \quad . \quad (67)$$

where c denotes the ordinary stress for tension alone. The stress per unit of area would be

$$b = \frac{\max B}{F} = \frac{c}{1 - \phi} \quad . \quad . \quad . \quad . \quad (68)$$

this formula coincides with (64) for $c = \frac{u}{m}$.

IX.—EXAMPLES.

For a better comprehension of the various methods of determining dimensions some examples shall be calculated.

As almost all Authors have left liability to buckling unnoticed, it will remain excluded here. It may be taken into account in a way which is for all methods analogous to that described in the Author's previous communication for the Launhardt and Weyrauch system.[1] The latter gave for tension or compression only without liability to buckling

$$b = v\,(1 + m\,\phi); \qquad F = \frac{\max B}{v\,(1 + m\,\phi)}; \quad . \quad . \quad (69)$$

and for alternate tension and compression

$$b = v\,(1 + n\,\phi); \qquad F = \frac{\max B}{v\,(1 + n\,\phi)} \quad . \quad . \quad (70)$$

On the authority of Wöhler's experiments for iron the values were adopted $v = 700$, $m = n = \frac{1}{2}$, whence generally

$$b = 350\,(2 + \phi) \qquad F = \frac{\max B}{350\,(2 + \phi)} \quad . \quad . \quad (71)$$

As, however, the choice of constants depends on the constructive material, the object of the structure and experience of the designer, no great weight was attached to the numerical values, and, for example, the less favourable data

$$b = 320\,(2 + \phi) \qquad F = \frac{\max B}{320\,(2 + \phi)} \quad . \quad . \quad (72)$$

were also given.

Example 1.—For the boom of a girder-bridge which is always in tension (or always in compression), the load varies between the maximum value max B = 30,000 and the minimum value, due to the fixed load only, min B = 10,000 kilograms. The requisite net sectional area is to be determined.

---

[1] Minutes of Proceedings Inst. C.E., vol. lxiii., p. 290.

(1) By the old method with $b = 700$ there follows—

$$F = \frac{30{,}000}{700} = 42 \cdot 86 \text{ square centimetres.}$$

(2) By Gerber's method with $B_e = 10{,}000$, $B_v = 20{,}000$ from (8)—(10).

$$\phi = \frac{10{,}000}{1 \cdot 5 \times 20{,}000} = \tfrac{1}{3}.$$

$$\delta = \tfrac{1}{4} \left(3 + \sqrt{16 \times \tfrac{1}{9} + 16 \times \tfrac{1}{3} + 13}\right) = 1 \cdot 871.$$

$$F = \frac{1 \cdot 5 \times 1 \cdot 871 \times 20{,}000}{1{,}600} = 35 \cdot 08 \text{ square centimetres.}$$

(3) By Schäffer's method with $B_0 = 10{,}000$, $B_1 = 20{,}000$, $B_2 = 0$ from (23)—(25).

$$\xi = \frac{1 \cdot 5 \times 20{,}000}{1 \cdot 5 \times 20{,}000 + 10{,}000} = \tfrac{3}{4}.$$

$$\frac{a}{m} = \frac{-3 \times \tfrac{3}{4} + \sqrt{13 \times \tfrac{9}{16} - 16 \times \tfrac{3}{4} + 16}}{(2 - \tfrac{3}{4})^2} \, 1{,}600 = 1{,}140.$$

$$F = \frac{1 \cdot 5 \times 20{,}000 + 10{,}000}{1{,}140} = 35 \cdot 08 \text{ square centimetres.}$$

(4) By Winkler's method with $B_0 = 10{,}000$, $B_1 = 20{,}000$, $B_2 = 0$ from (32).

$$F = \frac{10{,}000}{1{,}400} + \frac{20{,}000}{590} = 41 \cdot 04 \text{ square centimetres.}$$

(5) By Seefehlner's method with $\phi = 10{,}000 : 30{,}000 = \tfrac{1}{3}$ from (44).

$$F = \frac{4 - \dfrac{1 \cdot 6}{3}}{2 + \tfrac{1}{3}} \cdot \frac{30{,}000}{1{,}200} = 37 \cdot 14 \text{ square centimetres.}$$

(6) By Launhardt and Weyrauch's method with $\phi = \tfrac{1}{3}$ from (71).

$$F = \frac{30{,}000}{350\,(1 + \tfrac{1}{3})} = 36 \cdot 73 \text{ square centimetres.}$$

(7) By Ritter's method with $\phi = \frac{1}{3}$ from (48).

$$F = \frac{30{,}000\,(2 - \frac{1}{3})}{1{,}200} = 41 \cdot 67 \text{ square centimetres.}$$

(8) By Lippold's method from (51).

$$F = \frac{2 \times 30{,}000 - 10{,}000}{1{,}300} = 38 \cdot 46 \text{ square centimetres.}$$

(9) By Clericetti's method with $B_0 = 10{,}000$, $B_1 = 20{,}000$ from (60).

$$F = \frac{10{,}000 + 2 \times 20{,}000}{1{,}250} = 40 \cdot 00 \text{ square centimetres.}$$

Example 2.—For a member of a bridge always in tension (or always in compression) the greatest load max $B = 30{,}000$, the smallest min $B = 10{,}000$, as above, but the fixed load $B_0 = 20{,}000$ kilograms. The net sectional area is to be determined,

(1) By the old method as above, $F = 42 \cdot 86$ square centimetres.

(2) According to Gerber with $B_e = 20{,}000$, and $B_v = 10{,}000$.

$$\phi_1 = \frac{20{,}000}{1 \cdot 5 \times 10{,}000} = \frac{4}{3}.$$

$$\delta_1 = \frac{1}{4}\left(3 + \sqrt{16 \times \frac{16}{9} + 16 \times \frac{4}{3} + 13}\right) = 2 \cdot 731.$$

$$F_1 = \frac{1 \cdot 5 \times 2 \cdot 731 \times 10{,}000}{1{,}600} = 25 \cdot 60,$$

then with $B_e = 20{,}000$, $B_v = -\,10{,}000$.

$$\phi_2 = \frac{20{,}000}{1 \cdot 5 \times 10{,}000} = -\frac{4}{3}.$$

$$\delta_2 = \frac{1}{4}\left(3 + \sqrt{16 \times \frac{16}{9} - 16 \times \frac{4}{3} + 13}\right) = 1 \cdot 871.$$

$$F_2 = \frac{1 \cdot 5 \times 1 \cdot 871 \times 10{,}000}{1{,}600} = 17 \cdot 54,$$

and together

$$F = 25 \cdot 60 + 17 \cdot 54 = 43 \cdot 14 \text{ square centimetres.}$$

(3) According to Schäffer with $B_0 = 20,000$, $B_1 = 10,000$, $B_2 = 10,000$ from (23)—(25).

$$\xi = \frac{1 \cdot 5\,(10,000 + 10,000)}{1 \cdot 5 \times 10,000 + 20,000} = \frac{6}{7}.$$

$$\frac{a}{m} = \frac{-\,3 \times \frac{6}{7} + \sqrt{13 \times \frac{36}{49} - 16 \times \frac{6}{7} + 16}}{(2 - \frac{6}{7})^2}\,1,600 = 1,055.$$

$$F = \frac{1 \cdot 5 \times 10,000 + 20,000}{1,055} = 33 \cdot 18 \text{ square centimetres.}$$

(4) According to Winkler with $B_0 = 20,000$, $B_1 = 10,000$, $B_2 = 10,000$ from (32).

$$F = \frac{20,000}{1,400} + \frac{10,000}{590} + \frac{10,000}{1,300} = 38 \cdot 93 \text{ square centimetres.}$$

(5) According to Seefehlner as above $F = 37 \cdot 14$ sq. cent.
(6) „ „ Launhardt and Weyrauch $F = 36 \cdot 73$ „ „
(7) „ „ Ritter as above $F = 41 \cdot 67$ „ „
(8) „ „ Lippold „ $F = 38 \cdot 46$ „ „
(9) „ „ Clericetti with $\phi = \frac{1}{3}$ $F = 40 \cdot 00$ „ „

Example 3.—A piece sustains a maximum tensile load of max $B = 40,000$ and a maximum compression of max $B_1 = 20,000$, while the fixed load in tension is $B_0 = 10,000$ kilograms. The net sectional area is to be determined.

(1) According to the old method, with $b = 700$

$$F = \frac{40,000}{700} = 57 \cdot 14 \text{ square centimetres.}$$

(2) According to Gerber from (8)—(10) with $B_e = 10,000$, $B_v = 30,000$.

$$\phi_1 = \frac{10,000}{1 \cdot 5 \times 30,000} = \frac{2}{9}.$$

$$\delta_1 = \frac{1}{4}\left(3 + \sqrt{16 \times \frac{4}{81} + 16 \times \frac{2}{9} + 13}\right) = 1 \cdot 791.$$

$$F_1 = \frac{1 \cdot 5 \times 1 \cdot 791 \times 30,000}{1,600} = 50 \cdot 37$$

Then with $B_e = 10{,}000$, $B_v = -30{,}000$.

$$\phi_2 = -\frac{10{,}000}{1\cdot 5 \times 30{,}000} = -\frac{2}{9}.$$

$$\delta_2 = \tfrac{1}{4}\left(3 + \sqrt{16 \times \tfrac{4}{81} - 16 \times \tfrac{2}{9} + 13}\right) = 1\cdot 550.$$

$$F_2 = \frac{1\cdot 5 \times 1\cdot 550 \times 30{,}000}{1{,}600} = 43\cdot 59;$$

and together

$$F = 50\cdot 37 + 43\cdot 59 = 93\cdot 96 \text{ square centimetres.}$$

(3) According to Schäffer with $B_0 = 10{,}000$, $B_1 = 30{,}000$, $B_2 = 30{,}000$ from (23)—(25).

$$\xi = \frac{1\cdot 5\,(30{,}000 + 30{,}000)}{1\cdot 5 \times 30{,}000 + 10{,}000} = \frac{18}{11}.$$

$$\frac{a}{m} = \frac{-3 \times \frac{18}{11} + \sqrt{13 \times \frac{324}{121} - 16 \times \frac{18}{11} + 16}}{\left(2 - \frac{18}{11}\right)^2}\, 1{,}600 = 648.$$

$$F = \frac{1\cdot 5 \times 30{,}000 + 10{,}000}{648} = 84\cdot 88 \text{ square centimetres.}$$

(4) According to Winkler with $B_0 = 10{,}000$, $B_1 = 30{,}000$, $B_2 = 30{,}000$ from (32)

$$F = \frac{10{,}000}{1{,}400} + \frac{30{,}000}{590} + \frac{30{,}000}{1{,}300} = 81\cdot 07 \text{ square centimetres.}$$

(5) According to Seefehlner with $\phi = -20{,}000 : 40{,}000 = -\frac{1}{2}$ from (44)

$$F = \frac{4 + \frac{1\cdot 6}{2}}{2 - \frac{1}{2}}\,\frac{40{,}000}{1{,}200} = 106\cdot 67 \text{ square centimetres.}$$

(6) According to Launhardt and Weyrauch with $\phi = -\frac{1}{2}$ from (71)

$$F = \frac{40{,}000}{350\,(1 - \frac{1}{2})} = 76\cdot 19 \text{ square centimetres.}$$

(7) According to Ritter with $\phi = -\frac{1}{2}$ from (50)

$$F = \frac{40{,}000 \times 0\cdot 5}{(1 + 0\cdot 5 - \sqrt{1 + 0\cdot 5^2})\ 600} = 87\cdot 26 \text{ square centimetres.}$$

(8) According to Lippold from (52)

$$F = \frac{2 \times 40{,}000 + 20{,}000}{1{,}300} = 76\cdot 92 \text{ square centimetres.}$$

(9) According to Clericetti with $B_0 = 10{,}000$, $B_1 = 30{,}000$, $B_2 = 30{,}000$, from (62).

$$F = \frac{30{,}000 + 30{,}000 + 5{,}000}{650} = 100\cdot 00 \text{ square centimetres.}$$

Example 4.—A piece sustains a maximum tensile load max $B = 40{,}000$, and a maximum compressive load max $B^1 = 20{,}000$, which latter, however, at the same time, represents the fixed load. The net sectional area is to be determined.

(1) According to the old method, as above, $F = 57\cdot 14$ square centimetres.

(2) According to Gerber with $B_e = -20{,}000$, $B_v = 60{,}000$ from (8)—(10)

$$\phi = -\frac{20{,}000}{1\cdot 5 \times 60{,}000} = -\frac{2}{9}.$$

$$\delta = \tfrac{1}{4}\left(3 + \sqrt{16 \times \frac{4}{81} - 16 \times \frac{2}{9} + 13}\right) = 1\cdot 550.$$

$$F = \frac{1\cdot 5 \times 1\cdot 550 \times 60{,}000}{1{,}600} = 87\cdot 19 \text{ square centimetres.}$$

(3) According to Schäffer with $B_0 = 20{,}000$, $B_1 = 60{,}000$, $B_2 = 0$ from (23)—(25)

$$\xi = \frac{1\cdot 5 \times 60{,}000}{1\cdot 5 \times 60{,}000 - 20{,}000} = \frac{9}{7}.$$

$$\frac{a}{m} = \frac{-3 \times \frac{9}{7} + \sqrt{13 \times \frac{81}{49} - 16\,\frac{9}{7} + 16}}{\left(2 - \frac{9}{7}\right)^2}\,1{,}600 = 803.$$

$$F = \frac{1\cdot 5 \times 60{,}000 - 20{,}000}{803} = 87\cdot 18 \text{ square centimetres.}$$

(4) According to Winkler with $B_0 = 20{,}000$, $B_1 = 60{,}000$, $B_2 = 0$ from (32).

$$F = -\frac{20{,}000}{1{,}400} + \frac{60{,}000}{590} = 87 \cdot 41 \text{ square centimetres.}$$

(5) According to Seefehlner as above

$F = 106 \cdot 67$ square centimetres.

(6) According to Launhardt and Weyrauch as above

$F = 76 \cdot 19$ square centimetres.

(7) According to Ritter as above $F = 87 \cdot 26$ square centimetres.

(8) „ „ Lippold „ „ $F = 76 \cdot 92$ „ „

(9) „ „ Clericetti with $B_0 = 20{,}000$, $B_1 = 60{,}000$, $B_2 = 0$ from (62).

$$F = \frac{60{,}000 + 10{,}000}{650} = 107 \cdot 69 \text{ square centimetres.}$$

X. TABLES.

The following Tables are intended to show in a concise form how the admissible stress per square centimetre for iron bridges varies according to the various methods of determining dimensions previously demonstrated. As hitherto, for tension or compression only

$$\phi = \frac{\min B}{\max B} \qquad \phi_0 = \frac{B_0}{\max B},$$

and for alternate tension and compression

$$\phi = -\frac{\max B^1}{\max B} \qquad \phi_0 = \pm \frac{B_0}{\max B}.$$

In the latter case the upper or lower sign is applicable according as the fixed load (numerical value B_0) is in the same or opposite sense as the higher limiting load (numerical value max B).

In Table A, ϕ_0 may vary from ϕ ($B_0 = \min B$) to 1 ($B_0 = \max B$), for instance for the booms of girder bridges

$$\phi = \phi_0 = \frac{\min B}{\max B} = \frac{p}{q} \quad . \quad . \quad . \quad (73)$$

where p is the weight of the girder itself, and $q = p + z$ the total load on the latter per unit of length. Up to spans of 100 metres ϕ may attain about the value $\frac{1}{2}$. For comparison the values are also added of

$$b = \frac{p + z}{p + 3z} 1{,}600 = \frac{1{,}600}{3 - 2\phi} \quad . \quad . \quad . \quad (74)$$

which are those assumed to be admissible, as early as the year 1863, by Gerber, in calculating the Mainz railway bridge.[1]

TABLE A.—FOR TENSION OR COMPRESSION ONLY.

Method.	Formula.	ϕ_0.	$\phi = 0$.	$\phi = \frac{1}{4}$.	$\phi = \frac{1}{2}$.	$\phi = \frac{3}{4}$.	$\phi = 1$.
Old . . .	$b = c$	*ad lib.*	700	700	700	700	700
Mainz Bridge	(74)	ϕ	533	640	800	1,067	1,600
Gerber . .	(13).	ϕ	646	794	998	1,271	1,600
	(12)	$\frac{\phi + 1}{2}$	584	670	741	1,333	1,600
	(13)	1	703	891	1,140	1,402	1,600
Schäffer . .	(29)	ϕ	646	794	998	1,271	1,600
	(29)	$\frac{\phi + 1}{2}$	680	845	1,067	1,333	1,600
	(29)	1	704	892	1,140	1,402	1,600
Winkler . .	(38)	ϕ	592	692	832	1,044	1,400
	(38)	$\frac{\phi + 1}{2}$	632	733	871	1,074	1,400
	(38)	1	678	779	914	1,106	1,400
Seefehlner .	(43)	*ad lib.*	680	750	937	1,178	1,500
Launhardt & Weyrauch .	(71) (72)	*ad lib.* "	700 640	787 720	875 800	962 880	1,050 960
Ritter. . .	(47)	*ad lib.*	600	686	800	960	1,200
Lippold . .	(53)	*ad lib.*	650	743	867	1,040	1,300
Clericetti. .	(61)	ϕ	625	714	833	1,000	1,250

In Table B, ϕ_0 may vary: (1) when the fixed load is of the same kind as the upper limiting load, from 0 to 1, and (2) when the fixed load is of the opposite kind to the upper limiting load from 0 to ϕ.

---

[1] Gerber in Zeitschrift des Vereines deutscher Ingenieure, 1865.

TABLE B.—ALTERNATE TENSION and COMPRESSION.

Method.	Formula	ϕ_0.	$\phi = 0$.	$\phi = -\frac{1}{4}$.	$\phi = -\frac{1}{2}$.	$\phi = -\frac{3}{4}$.	$\phi = -1$.
Old . . .	$b = c$	*ad lib.*	700	700	700	700	700
Gerber . .	(15)	ϕ	646	538	459	399	351
	(14)	$\frac{\phi}{2}$	646	513	424	361	315
	(14)	0	646	517	431	369	323
	(14)	$\frac{1}{2}$	584	486	412	358	315
	(15)	1	703	569	474	404	351
Schäffer . .	(29)	ϕ	646	538	459	399	351
	(29)	$\frac{\phi}{2}$	646	544	464	403	354
	(29)	0	646	550	469	406	356
	(29)	$\frac{1}{2}$	680	563	473	405	354
	(29)	1	704	573	474	404	351
Winkler . .	(38)	ϕ	592	518	460	413	376
	(38)	$\frac{\phi}{2}$	592	525	471	428	391
	(38)	0	592	556	523	495	408
	(38)	$\frac{1}{2}$	632	591	555	522	427
	(38)	1	678	636	590	553	448
Seefehlner .	(43)	*ad lib.*	600	477	375	289	214
Launhardt &	(71)	*ad lib.*	700	612	525	437	350
Weyrauch .	(72)	"	640	560	480	400	320
Ritter. . .	(49)	*ad lib.*	600	526	458	400	321
Lippold . .	(53)	*ad lib.*	650	578	520	473	433
Clericetti. .	(63)	ϕ	650	473	371	306	260
	(63)	$\frac{\phi}{2}$	650	495	400	335	289
	(63)	0	650	520	433	371	325
	(63)	$\frac{1}{2}$	520	433	371	325	289
	(63)	1	433	371	325	289	260
American .	(66)	*ad lib.*	700	560	467	400	350

From both the preceding Tables the inaccuracy of Gerber's division of the difference of stress (I) will be apparent, as it is impossible that the admissible stress *b* should vary with ϕ_0 in the irregular manner shown.

The remarkable values obtained by Clericetti in Table B are due to the assumption mentioned towards the end of VIII. with regard to the actual stresses with a suddenly applied load. If this assumption is corrected and formula (66) accepted, the values given in Table B as "American" are the result (compare VIII.).

If two pieces, subjected only to tension or only to compression, sustain equal maximum loads, max B, and equal minimum loads, min B, according to Table A and examples (1), (2), (IX.), the formulæ of Schäffer and Winkler give the smallest sectional area for that piece which sustains the greatest fixed load. The same remark applies to Gerber, in so far as the influence of the division of the difference of the load previously referred to is not predominant. This is due to the fact that the live load is introduced into the calculation by the Authors named, multiplied with a factor *n*, for the purpose of taking separately into account the influence of impact.

Of course none of the methods of calculation reviewed are intended to give a rigid law for determining dimensions, but merely as guides from which the practical engineer will deviate in one direction or another if there are reasons for so doing.

As formerly the value $b =$ constant was taken, but with reference to particularly unfavourable conditions lower values also were adopted, so the methods reviewed assume $b = f(x, y \ldots)$, but leave free play for the recognition of any special circumstances. For calculating the parts of machinery it has latterly been thought desirable to systematically utilise Wöhler's results,[1] experience having led to the adoption of perfectly different admissible stresses for different ways of applying loads, which, however, in the cases of static tension, tension alternating with zero, and alternations of equal tension and compression confirmed Wöhler's ratio of 3 : 2 : 1 given by formulæ (71) (72). Undoubtedly all the new methods have their faults, but the method hitherto in use is quite indefensible.

---

[1] C. Bach: "Die Maschinenelemente, ihre Berechnung und Construction mit Rücksicht auf die neueren Versuche." Stuttgart, 1881.

(*Paper No. 1896.*)

"Modern Corn-Milling."

By Professor Cesare Saldini, of Milan.

Translated by Walter Hampden Thelwall, Assoc. M. Inst. C.E.

The mechanism of corn-mills is just now undergoing a great and radical transformation. The old millstones, which served men for twenty centuries, will soon be banished from mills. New apparatus and new processes have been devised, and their construction is such that they will henceforward recommend themselves to every one. Nowadays, indeed, no one doubts that the process of high grinding, so well explained in the Papers[1] which recently appeared in the Minutes of Proceedings, represents real and useful progress; nor does any one doubt that roller-mills will prove, in all respects, better than stone-mills. It may be an open question whether it would not be better to adopt the new methods partially, rather than in their entirety; but it cannot but be admitted that they possess very great merits and advantages, and have been well thought out. The expediency of complicating the arrangements of the mills in order to obtain a constantly improving quality of flour, when the refinement of the public taste does not as yet demand it, may be called in question; the necessity, from an economical point of view, of producing an extensive series of flours of different degrees of colour, purity, and nourishing power, may be doubted, on account of the risk of the finer qualities being too expensive, the coarser too dark and unsaleable. An apt hand and sound judgment are required for the selection of the process best adapted to ever-changing local requirements, so as not to run the risk of compromising a business for the sake of theoretical perfection, but it is certain that more or less complicated high-grinding constitutes the best means yet invented of removing the flour from the grain, and that which yields the highest return in quantity and quality. It is certain that the force of the stones, which entirely and at once crushes the grains of corn, ought to be superseded by the process of repeated passages between grooved

---

[1] Minutes of Proceedings Inst. C.E., vol. lxx., pp. 162 *et seq.*

rollers, which gradually opens and breaks them in pieces, and frees them from impurities. Then the work of the grinding rollers, which are either finely cut, or made of porcelain with a naturally rough surface, divests the coarse bran of those valuable particles which it still contains, a work which the millstone is unfitted to accomplish.

This preparatory process of breaking the grain and removing the residuum of semolina, has the effect of producing, besides the bran, two or three qualities of flour of inferior grade, which are rich in starch and dirty, and the production of which in the passage between the rollers cannot be prevented, and, which is of more importance, yields an assortment of middlings and semolinas which, after being conveniently assorted, are converted into flour between smooth rollers of chilled iron or porcelain, or else between stones. This last use of stones should not be forgotten, for it supplies a means of utilising the old material of a mill in course of transformation, without in any way detracting from the value of the new methods.

Some persons object that the high-grinding systems are far from possessing that simplification of labour which was claimed for them on their first introduction, and which, in truth, they have not attained; and they add that however simple an arrangement of grinding is adopted, with few (three or four) breaks, few (two) cleaning operations, and few operations of actual grinding, a series of different products will be obtained from the best flour, beautifully white and of golden brilliancy, down to the last and darkest coloured flours. These products, varying so much in character, cannot obtain purchasers who will give the prices required, and they conclude that the conversion of an old mill is often premature. The Author considers that this reasoning is erroneous, for if it is certain that high-grinding gives a greater return in flour; if it is certain that each of the various qualities obtained is specifically purer and more uniform than those which are produced by low grinding, why should not the different qualities obtained be carefully mixed together? Why should not the five, six or eight kinds obtained, be successfully reduced to two, three, or whatever number has been habitually used in the past? The kinds formerly used will still be obtained, with the advantage that they will undoubtedly be better than formerly, in consequence of the perfect manipulation which the flour has undergone.

Is it not a fact that the benefit of the larger yield of the grain in quantity, and the more satisfactory results in regard to quality, are always obtained? Is it not, then, a fact, that such advantages are independent of the number of kinds produced when the effective methods of mixing them are thoroughly understood? It would certainly seem so. It is certain that the necessary expenditure of new arrangements, when compared with the advantages in working, can never be a serious argument against the adoption of new methods and apparatus.

If, leaving for the present the essential parts of the modern revolution in corn-mills, attention is paid to the improvements necessarily introduced into the machines intended to effect it, and especially to the roller-mills, it will be seen that the perfection which has been attained in the last few years is in the highest degree satisfactory.

Roller-mills, like all other machines, have their history of exaggerated hopes and exaggerated disappointments. Originated in about the year 1830, they were successively abandoned and taken up again, by their constructors, up to about 1870. Their success dates from this latter period. The firm of Ganz, of Budapest, have been the foremost in working to obtain this success, with their spirally grooved cylinders of chilled iron, intended for breaking up the grain, and their smooth cylinders, also of chilled iron, for grinding; next Wegmann, of Zurich, with his porcelain cylinders for stripping and grinding middlings, at which, for the last twenty years, he has worked with exemplary perseverance and ever-increasing success; and many other inventors.

The doubts which five or six years ago disturbed the minds of millers and machinists are disappearing. The obscurity which surrounded their judgment in regard to the selection of the right diameters for the cylinders and the method of grooving them, as well as their relative velocities, is fading away. It is now definitely settled that the diameter should not be very large (not more than about 1 foot 6 inches), as otherwise the advantage which rollers possess over stones in shortening the passage of the corn between the grinding surfaces would be lost. It is settled that such diameter should be chosen as will give the highest output without an excessive duration of contact between the grain and the surfaces of the cylinders. It is a settled principle to cause the two rollers, of a pair, to revolve with different circumferential velocities, either by having equal diameters and different rates of revolution, or by equal rates of revolution and different diameters, varying from the ratio of 1 to 1·5 for the first pair, up to 1 to 4 for the last; and it is also settled that the grooving should increase in fineness from the first to the last pair as the dressing of the coarse middlings proceeds.

From the first of these principles it follows that one of the cylinders, the one which has the slower motion, acts as feeder to the other, which runs at a higher speed. From the second is derived the necessity of using rollers with finer and finer grooves as the fragments derived from the successive breakings get smaller, till the middlings are reached, which can be dressed with rollers of 9-inches diameter, carrying from nine hundred to one thousand grooves, or even with porcelain rollers, whose natural roughness takes the place of the grooves.

Finally, it is placed beyond doubt that the grinding, of the semolina (*gries*) and of the middlings (*dunst*) previously cleaned, can be conveniently effected either by stones or by Wegmann's machines with porcelain cylinders, or with rollers of smooth chilled iron. And it is no little result to have determined so many doubtful points, and to have set out on this high road of which in the past there was not even a trace, insomuch as only to-day can corn-grinding aspire to take its place among the industries which are nearest perfection, and to a certain extent complete.

The Author does not consider it necessary to say anything about the other apparatus connected with milling. Centrifugal sifting-machines have been well received on account of the small space they occupy and their great efficiency; and still more popular are those machines for cleaning the bran, which satisfy the two conditions of extreme simplicity of construction and efficiency in production. It is only needful to state that these results have been achieved.

In Italy the principal millers have begun to rouse themselves, and were it not for the tax upon grinding, which, with its intricate fiscal laws, is fatal to any kind of progress, a further advance would certainly have been made than obtains at present. On all sides the necessity of welcoming new methods is becoming imperative, and it is to be expected that the abolition of the above mentioned tax with the year 1883 will give an open field for boldly renewing all old-fashioned plant. Millers of the middle and lower ranks see their flour discredited, and daily find themselves face to face with buyers who give the preference to flour from roller-mills, and the same classes of flour already fetch two different prices according as they are produced from low or high grinding. On the other hand, the taste of the public is rapidly learning to appreciate improved products, and before many years have passed Italy too will have attained a satisfactory position in regard to the industry of corn-milling.

(*Paper No. 1900.*)

"Current-Meter Observations in the Thames."

By Professor WILLIAM CAWTHORNE UNWIN, B. Sc., M. Inst. C.E.

[NOTE.—The Amsler Laffon current-meter being graduated to the metric scale, it would be impracticable to convert the following values into English measures of the necessary exactness, without using several places of decimals; the metric values are therefore retained throughout. 1 metre = 3·281 feet; 1 cubic metre = 35·316 cubic feet or 220 gallons.—SEC. INST. C.E.]

THE following Paper contains the record of a series of current-meter observations, partly in the tidal, partly in the non-tidal, waters of the Thames.

Those in the tidal part of the river were undertaken with a view of examining the action of the Amsler Laffon current-meter in deep water, and also to ascertain how far useful observations could be made in a tidal stream. The experiments were made in January 1882, when the shortness of the days somewhat restricted the operations, but when the comparative absence of traffic was favourable to experiment. The observations were intended only as a preliminary to more complete experiments, but as the results are not without interest, and as the Author cannot at present continue the series, they are briefly recorded here. The other observations contain a gauging of the Upper Thames in the high flood of October 1882.

The meter already described to the Institution[1] is suspended by a wire, with a weight of 40 kilograms below it. A conical rudder keeps it directed to the stream, and an electrical signal is given at each one hundred revolutions. The facility of changing the position of a meter thus suspended is very great; the only objection is that the instrument can sway about, and does not register strictly the component of the water's motion normal to the plane of section. So far as the Author could observe, the meter when near the surface, held its position in the water very steadily. But allowing the objection, it may be well to consider what is the probable magnitude of the error due to the oscillation of the axis of the meter.

If the current is assumed to flow normally to the plane of the section where a gauging is made, and the meter is held at an

---

[1] Minutes of Proceedings Inst. C.E., vol. lxvi., p. 32.

angle of 20° with the normal to that section, the velocity registered would be less than the true velocity, in the ratio of cos 20° to 1; that is, the velocity registered would be 0·94 of the true velocity, an error of 6 per cent. But this is not what would happen with a suspended meter, in water somewhat unsteady. The meter in that case would swing to and fro over an angle more or less large. Suppose the meter swung regularly over an angle of 20° on each side of the normal to the section, or 40° in all, the mean error would not exceed 3 per cent.; and in many cases, at all events, an error of 3 per cent. would not be of any very serious moment. So far as the Author could observe, the swing of the meter is certainly less than that here assumed.

OBSERVATIONS IN THE TIDAL PART OF THE THAMES AT PUTNEY.

On four days the meter was in use for twelve and a half hours altogether, and during that time two hundred and seventy-five velocities were measured, on a single vertical, at depths ranging from 2·3 to 7·3 metres. Consequently one observation was made and recorded by a single observer every three minutes. This includes all the time necessary to shift the position of the instrument, and all delays due to passing tugs and barges. No other form of meter, in the Author's opinion, would permit observations to be made with so great a rapidity in a deep stream. In a tidal river, where the velocity is constantly varying with the change of depth and surface slope, this rapidity is very valuable.

The following Tables contain the observations made during one complete period of flow of the tide, and two periods of ebb. They are selected, because taken together they permit curves to be drawn for the whole period of a tide. The tidal times at London Bridge, and the depth of water on the sill of the Shadwell entrance to the London Docks, on the days of these observations, as given in Mr. Roberts's Table, were as follows:—

	Time of High Water, London Bridge.	Depth on Dock Sill.
January 21	3h. 16m. P.M.	29 ft. 5 ins.
„ 13	7h. 5m. A.M.	25 ft. 6 ins.
„ 10	5h. 11m. A.M.	27 ft. 4 ins.

River-Section.—The river-section was taken by soundings from a boat, the position of the boat being noted by sextant-angles. The section having been plotted, the area was calculated for each metre rise of the water-surface from low to high water. From these figures the diagram, Plate 6, Fig. 1, was plotted, giving the area for any depth of water, at the vertical on which the velocities were taken.

The greatest depth of water at the site of the gaugings on January 21st was 23 feet. At Chelsea, on the same day, the tide rose 1 foot 8 inches above T. H. W. Consequently the bed of the river at the section was about 21 feet 4 inches below T. H. W.

Velocity-Observations.—The meter was fixed on a steam-launch, moored with two anchors, always at the same spot in the river, opposite Craven Cottage. The velocities were taken on the same vertical, but at different depths below the surface.

The first Table gives the observations during the flow of the

JANUARY 21.—TIDE EBBING at FIRST, then FLOWING, FINALLY EBBING AGAIN, so that the OBSERVATIONS COVER an ENTIRE PERIOD of TIDAL FLOW. NO WIND.

Time. P.M.	Depth of River.	Depth of Meter below Surface.	Velocity per Second.	Time. P.M.	Depth of River.	Depth of Meter below Surface.	Velocity per Second.
H. M.	Metres.	Metres.	Metre.	H. M.	Metres.	Metres.	Metre.
12 5	2·20	0·00	0·6659	2 8	..	0·50	0·8495
12 7	..	0·00	0·6285	2 15	..	0·00	0·8466
12 9	..	0·50	0·4849	2 18	5·57	5·27	0·7180
12 15	2·28	1·98	0·2642	2 25	5·83	5·53	0·6946
12 19	,,	1·00	0·2001	2 29	..	4·15	0·8029
12 23	..	0·50	0·2001	2 31	..	2·76	0·8657
12 27	2·53	2·23	0·0000	2 34	..	1·38	0·8940
12 35	Commencement of flow			2 36	..	0·50	0·9018
12 45	..	0·50	0·1940	2 39	..	0·00	0·9119
12 49	..	0·50	0·3534	2 41	6·17	5·87	0·5244
12 51	3·34	3·04	0·2769	2 45	..	2·93	0·8495
12 53	..	1·52	0·4000	2 48	..	0·50	0·8579
12 55	..	0·50	0·4155	2 56	6·55	6·25	0·6542
12 58	3·54	3·24	0·3079	3 0	..	3·12	0·7863
1 0	..	1·62	0·5501	3 2	..	0·50	0·8120
1 3	..	0·50	0·5752	3 15	6·95	6·65	0·5717
1 5½	3·68	3·38	0·4421	3 19	..	0·50	0·7643
1 8	..	1·69	0·6039	3 30	7·11	6·81	0·4602
1 11	..	0·50	0·6372	3 34	..	0·50	0·6699
1 14	3·87	3·57	0·4515	3 38	..	0·50	0·6797
1 17	..	1·79	0·6647	3 44	..	0·50	0·6462
1 20	..	0·50	0·7455	3 46	7·29	6·99	0·4339
1 23½	4·12	3·82	0·5404	3 51	..	3·49	0·4842
1 28	..	1·91	0·7188	3 56	..	0·50	0·5207
1 31	..	0·50	0·7799	4 0	..	0·50	0·4155
1 37	4·50	4·20	0·6135	4 8	..	0·50	0·3160
1 39	..	2·10	0·7932	4 10	Water-level falling		
1 41	..	0·50	0·8267	4 19	Commencement of ebb		
1 45	4·67	4·37	0·5812	4 30	..	0·50	0·3079
1 47	..	3·28	0·7388	4 33	..	0·50	0·3160
1 49	..	2·19	0·8370	4 35	6·85	6·55	0·3040
1 51	..	1·09	0·8579	4 38	..	0·50	0·5030
1 54	..	0·50	0·8341	4 40	6·75	6·45	0·3946
1 56	..	0·00	0·7950	4 42	..	0·50	0·6054
1 58	5·15	4·85	0·5705	4 44	6·57	6·27	0·4306
2 1	..	3·63	0·8570	4 47	..	0·50	0·7518
2 4	..	2·42	0·8120	4 50	6·47	6·17	0·4602
2 6	..	1·21	0·8546	4 53	..	0·50	0·8357

tide, the velocities being generally taken in succession at 0·5 metre below the surface, at mid-depth, and at 0·3 metre above the bottom. The mean of these three velocities must be very near the mean velocity on the vertical.

Fig. 2 shows these observations plotted in curves. The surface and mid-depth curves are very regular, the latter falling pretty uniformly below the former, except at the beginning, where the observations are insufficiently numerous. The bottom-velocity curve is more irregular, but not excessively so, compared with observations in non-tidal rivers.

The ratio of the velocities at the three depths at which they were chiefly observed cannot be obtained accurately by averaging the numbers in the Table above, because the time-intervals are irregular. The following Table gives the velocities scaled off the curves at each twenty-minute interval, during the period of flow.

Time. P. M.	Velocity in Metres per Second.		
	At 0·5 metre below Surface.	At Mid Depth.	At Bottom.
12 40	0·130	0·170	0·130
1 0	0·510	0·550	0·340
1 20	0·745	0·680	0·505
1 40	0·820	0·795	0·600
2 0	0·840	0·820	0·585
2 20	0·870	0·840	0·710
2 40	0·890	0·855	0·535
3 0	0·820	0·785	0·640
3 20	0·760	0·665	0·535
3 40	0·670	0·550	0·440
4 0	0·415	0·330	0·250
4 20	0·000	0 000	0·000
Sum . . .	6·97	6·840	5·270

Hence it appears that the mid-depth-velocity is 0·98, and the bottom-velocity 0·75, of the velocity at 0·5 metre-below the surface. These proportions would not be unusual in a non-tidal river.

On January 13th the observations were taken in regular sets, the sequence being to take the velocities at—

0·5, 1·0, 2·0, 3·0, 4·0, bottom, 3·0, 2·0, 1·0, 0·5

metres below the surface. If the corresponding pairs of velocities in such a set are averaged, the result, since the intervals of time are not large, is approximately the velocity at the moment of the

JANUARY 13.—TIDE EBBING. LIGHT WIND UP-STREAM. TIME TAKEN USUALLY for 300 REVOLUTIONS OF METER.

Time. A.M.	Depth of River.	Depth of Meter below Surface.	Velocity per Second.	Time. A.M.	Depth of River.	Depth of Meter below Surface.	Velocity per Second.
H. M.	Metres.	Metres.	Metre.	H. M.	Metres.	Metres.	Metre.
8 47	5·02	0·50	0·7280	9 54	..	0·00	0·8357
8 49	..	4·72	0·5454	9 56	..	0·50	0·8117
8 55	..	0 50	0·7129	9 59	..	1·00	0·8341
8 58	..	1·00	0·7234	10 1	..	2·00	0·8144
9 0	..	2·00	0·6970	10 3	..	3·00	0·7121
9 2	..	3·00	0·6941	10 5	4·10	3·80	0·6348
9 7	4·92	4·62	0·5448	10 9	..	3·00	0·7265
9 14	4·86	4·56	0·5781	10 11	..	2·00	0·8029
9 15	..	3·00	0·7056	10 13	..	1·00	0·8421
9 17	..	2·00	0·7396	10 15	..	0·50	0·8117
9 18	..	1·00	0·8074	10 17	..	0·00	0·8047
9 20	..	0·50	0·7950	10 20	..	0·50	0·8446
9 22	..	0·00	0·8161	10 23	..	1·00	0·8526
9 24	..	0·50	0·8018	10 28	..	2·00	0·8341
9 25	..	1·00	0 8006	10 31	..	3·00	0·6833
9 29	..	2·00	0·8041	10 33	3·94	3·64	0·6509
9 32	..	3·00	0·7311	10 35	..	3·00	0·6887
9 37	4·49	4·19	0·6268	10 38	..	2·00	0·8446
9 46	..	3·00	0 7396	10 40	..	1·00	0·8507
9 48	..	2·00	0·8286	10 42	..	0·50	0·8657
9 50	..	1·00	0·8144	10 44	..	0·00	0·8482
9 52	..	0·50	0·8237				

observation of the bottom-velocity. Proceeding thus, the following Table has been formed:—

Time.	Velocity in Metres per Second.					
	At Surface.	Below Surface.				At 0·3 Metre above bottom.
		0·5 Metre.	1·0 Metre.	2·0 Metres.	3·0 Metres.	
H. M. H. M.						
8 47 to 8 49	..	0·728	..	..	..	0·545
8 55 „ 9 9	..	0·713	0·723	0·697	0·694	0·545
9 14 „ 9 22	0·816	0·795	0·807	0·740	0·706	0·578
9 24 „ 9 37	..	0·802	0·801	0·804	0·731	0·627
9 46 „ 9 54	0·836	0·824	0·814	0·829	0·740	..
9 56 „ 10 5	..	0·812	0·834	0·814	0·712	0·635
10 9 „ 10 17	0·805	0·812	0·842	0·803	0·727	..
10 20 „ 10 33	..	0·845	0·853	0·834	0·683	0·651
10 35 „ 10 44	0 848	0·866	0·851	0·845	0·689	..

On the whole the velocity increases at each position on the vertical with great regularity.

Taking the sum of the velocities at 0·5 metre below the surface, and at 0·3 metre above the bottom, for those times at which both were observed, the result is 4·695 and 3·581, the latter being

0·75 of the former, which exactly agrees with the ratio for the tide previously discussed. Fig. 3 gives the results of the Table above plotted in curves.

On January 10th the last portion of an ebb tide was observed, the velocities being taken usually at 0·5 metre below the surface, at mid-depth, and at 0·3 metre above the bottom. The following Table contains an abstract of these results, which are also plotted in Fig. 4. The curves are remarkably regular, considering the known unsteadiness of the water and the number of velocities obtained.

JANUARY 10, 1882.—BETWEEN PUTNEY and HAMMERSMITH. TIDE EBBING.

Time of Observation.	Velocity at Station in Mid-Stream in Metres per Second.				Depth of River in Metres.	—
	At Surface.	Below Surface.				
		0·5 Metre.	Mid-Depth.	0·3 Metre above Bed.		
H. M. H. M.						
11 16 to 11 24	..	0·9179	0·8998	0·7518	3·00	
11 29 „ 11 33	..	0·9142	0·8906	0·6845	2·90	
11 42 „ 11 48	..	0·9290	0·8191	0·6907	2·70	
11 50 „ 11 56	..	0·9367	0·8963	0·6978	2·70	
11 57 „ 12 2	..	0·9142	0·8926	0·7280	2·60	
12 8 „ 12 14	0·9142	0·9070	0·8790	0·7695	2·50	
12 22 „ 12 26	..	0·9444	0·8790	0·6706	2·50	
12 27 . .	..	0·9854	..	..	..	Greatest velocity.
12 37 „ 12 43	..	0·9215	0·9229	0·7013	2·50	
12 46 „ 12 50	..	0·9328	0·9070	0·7643	2·40	
12 52 „ 12 56	..	0·9661	0·9033	0·6845	2·35	
12 58 „ 1 2	..	0·9367	0·9290	0·6970	2·34	
1 8 „ 1 12	..	0·9444	0·8546	0·6621	2·34	
1 17 „ 1 22	..	0·9604	0·8926	0·7157	2·34	
1 30 „ 1 37	0·9267	0·8948	0·8657	0·6915	2·34	
1 45 „ 1 56	0·8770	0·9215	0·7772	0·5998	2·34	Ebb rapidly slackening.
1 58 „ 2 2	..	0·8144	0·7643	0·6233	2·34	
2 4 „ 2 9	..	0·7056	0·5945	0·5744	2·36	
2 11 . .	..	0·6408	..	..	..	
2 17 „ 2·22	..	0·4976	0·3482	0·3202	2·64	
2 24 . .	..	0·3758	..	..	..	
2 27 . .	..	0·2527	..	..	..	
2 34 . .	..	0·1218	..	..	2·90	
2 38 . .	..	0·0936	..	..	End of ebb.	

63 velocities taken in 202 minutes.

As the observations follow in regular sequence, it will be sufficient, in finding the ratio of the velocities at different depths, to take their sum in those cases where all three were observed. Proceeding thus the following sums are obtained :—

At 0·5 m. below surface.	At mid-depth.	At 0·3 m. above bottom.
15·96	14·92	12·03

Hence the mid-depth-velocity is 0·93, and the bottom-velocity 0·75 of the velocity at 0·5 metre below the surface; numbers which agree closely enough with those previously obtained.

Discharge of the River.—From each of the three sets of velocity-curves Figs. 2, 3, 4, the depth of the river, and the mean velocity on the gauging vertical has been scaled off for each twenty minutes of each tide. From Fig. 1 the corresponding area of the section of the stream has been obtained. For the discharge of the stream, the product of the area of section and mean velocity on the gauging vertical (which was near mid-stream) has been multiplied by 0·9, to allow for the diminution of velocity towards the sides. Thus is obtained the following Table. In fitting these three portions of tides together the times of high water at London

Date.	Tidal Time.	Depth of River.	Mean velocity observed on vertical V. Met. per. sec.	Section of River in square metres. Ω	Discharge in cubic metres per second 0·9 Ω V.	
1882.	H. M.	Metres.				
Jan. 21 .	-4 10	2·30	+0·40	250	90	
	3 50	2·60	+0·00	295	0	Flow begins.
	3 40	3·00	-0·15	357	- 48	
	3 20	3·60	-0·47	460	-195	
	3 0	4·00	-0·65	529	-310	
	2 40	4·50	-0·74	625	-416	
	2 20	5·20	-0·75	770	-520	
	2 0	5·60	-0·81	865	-631	
	1 40	6·20	-0·76	1,000	-684	
	1 20	6·65	-0·75	1,110	-749	
	1 0	7·00	-0·65	1,205	-705	
	0 40	7·30	-0·56	1,280	-645	
	0 20	7·15	-0·33	1,240	-368	
	0 0	7·00	-0·00	1,205	0	Ebb begins.
	+0 20	6·75	+0·47	1,130	478	
Jan. 13 .	+0 50	5·00	+0·64	731	421	
	1 10	4·70	0·67	665	401	
	1 30	4·60	0·75	640	432	
	1 50	4·30	0·77	585	405	
	2 10	4·00	0·76	529	362	
	2 30	4·00	0·77	529	366	
Jan. 10 .	5 20	3·00	0·86	357	276	
	5 40	2·70	0·81	310	226	
	6 0	2·60	0·84	295	223	
	6 20	2·50	0·83	280	209	
	6 40	2·50	0·85	280	214	
	7 0	2·34	0·86	260	201	
	7 20	2·34	0·86	260	201	
	7 40	2·34	0·80	260	187	
	8 0	2·36	0·73	263	173	
	8 20	2·64	0·39	300	105	
	8 40	2·90	0·00	340	0	Ebb ends.

Bridge have been used, and the actual times observed reduced to corresponding times of a single tide having high water at noon. When the direction of motion is up stream the velocity and discharge are marked negative.

Fig. 5 gives the figures in this Table plotted as a curve, the tidal hours being taken as abscisses and the discharges as ordinates, and a corresponding curve of river depth. The curve of discharge for the flow is extremely regular. Unfortunately the tide of January 13th was markedly lower than that of January 21st, and, as will be seen, the flow of upland water was greater on the former date. Hence the two curves do not join as well as would otherwise have been the case. The curve for January 10th follows fairly that of January 13th, the intermediate space being dotted.

Total Volume of Flow. Tidal and Upland Water.—The area of the curve from 3 hr. 50 min. till 0 hr. 0 min. of time is the total volume passing the section upwards during the flow of the tide. The area of the curve from 0 hr. 0 min. till 8 hr. 40 min. is the total volume flowing down stream during ebb. The difference of the two areas should represent the upland water, and there is therefore a check here on the general accuracy of the whole of the operations. The following are the results obtained by measuring the curves:—

Flow of Tide—

Mean discharge	431 cubic metres per second.
Duration of flow	230 minutes.
Total volume of flow	5,973,660 cubic metres.

Ebb of Tide—

Mean discharge	288 cubic metres per second.
Duration of ebb	520 minutes.
Total discharge	8,985,600 cubic metres.

Difference of ebb and flow, or upland water = 3,011,940 cubic metres.

This volume is equivalent to a discharge of 67 cubic metres per second by the river, during the whole period of a tide.

Upland Water.—Mr. J. Taylor, M. Inst. C.E., kindly furnished the Author with the following measurements of the water flowing down at Teddington on the days of the tidal observations:—

	Flow of Upland Water at Thames Ditton.
January 10	3,151 cubic feet per second.
„ 13	2,735 „ „
„ 21	1,606 „ „
Mean . . .	2,497 „ „

or 70 cubic metres per second. The agreement of this with the quantity found above is closer than could have been hoped for, especially as, instead of obtaining complete observations through a single tide, portions of three tides of somewhat unequal height have been combined.

It may be noted that the discharge of the river on the days of experiment was a flood-discharge, though not reaching probably one-seventh of the maximum flood-discharge in exceptionally high floods.

A discharge of 70 cubic metres per second during the whole period of a tide is equivalent to 102 cubic metres per second during the period of ebb. Hence at the section at Putney 10 miles below the limit of tidal action, the upland water amounted to $\frac{102}{283}$ of the whole flow during ebb. This ratio would no doubt rapidly diminish down stream. Using this ratio, the volume of upland water has been marked off on the diagram, Fig. 5.

GENERAL CONCLUSIONS.

(1.) The change of river-depth begins markedly earlier than the change of the direction of motion, at the turn of the tide.

(2.) The relation of the surface- and sub-surface-velocities is similar to that in an ordinary river at all periods of the tide.

(3.) The velocities could not be observed exactly at the turn of the tide, but in a very short period after the direction of motion had changed, the bottom water was in motion with a velocity bearing about the same ratio to the surface-velocity as at other periods of the tide.

(4.) The change of velocity at the turn of the tide is very rapid.

(5.) There are only about five minutes at the beginning of ebb and only about twenty minutes at the end of ebb, during which the river discharge is equal to or less than the upland water. That is for less than $\frac{19}{20}$ of the period of ebb the flow exceeds the upland water.

(6.) The discharge per second and velocity are markedly greater during flow than during ebb.

CURRENT-METER OBSERVATIONS AT OLD WINDSOR DURING A HEAVY FLOOD.

On October 30th and 31st, 1882, the river was flooded to very nearly the same height as the flood of 1875, which the Author gauged.[1] On the 31st a gauging was made in the straight reach

---

[1] Minutes of Proceedings Inst. C.E., vol. xlix., p. 156.

at Old Windsor with the result that the quantity of water passing down the river-channel amounted to nearly 8,000 cubic feet per second. This is one-third less than the amount found in 1875; but the section gauged at Albert Bridge in that year was so chosen that very little water escaped past the section without measurement. At the Old Windsor section the fields were deeply flooded over a great width on the left bank of the stream, and a large quantity of water escaped measurement.

The meter was fixed on a launch moored by an anchor upstream, and by a rope to each bank. The velocities were measured on five verticals, and usually the measurement was repeated three or four times in each position of the meter.[1] The following Table gives an abstract of the velocities observed:—

	Vertical I.	Vertical II.	Vertical III.	Vertical IV.	Vertical V.
Distance from river bank, met.	10·63	17·5	23·5	28·1	35·5
Total depth of river, metres..	5·3	6·3	5·75	4·8	3·9
Velocity at 0·5m. from surface	1·67	1·90	1·72	1·42	0·84
" 1·0	1·70	1·89	1·72	1·46	0·72
" 1·5	1·78	1·90	1·80	1·56	0·73
" 2·0	1·80	1·96	1·87	1·59	0·64
" 2·5	1·72	1·92	..	..	..
" 3·0	1·71	1·85	1·86	1·70	0·67
" 3·5	1·67	1·87	..	..	..
" 4·0	1·52	1·66	1·85	1·43	..
" 5·0	1·28	1·52	1·69	..	..
" 6·0	..	1·07	..	..	..
" 0·3m. above bed	1·28	1·07	1·40	1·45	..
Mean on vertical obtained from curve	1·62	1·71	1·76	1·53	0·71

This gives the bottom-velocity 0·77 of the surface-velocity, or rather the velocity at 0·5 metre below the surface. This ratio agrees with those given in the previous part of the Paper.

As the verticals are few, and are irregularly spaced, the method adopted for obtaining the discharge was as follows:—The vertical-velocity curves for the five verticals were drawn, and the mean velocity for each vertical obtained from these. The curves are given in Fig. 6. The mean velocities were set off as ordinates

---

[1] Mr. S. de Perrot and Mr. C. E. Jones assisted the Author in the gaugings.

at the proper positions on the river section, Fig. 7. Through the points so found the horizontal mean-velocity curve was drawn. The curve is so regular that no great error can arise from continuing it to the banks. The river-section was then divided into compartments, and the area of each multiplied by the mean velocity in each compartment gave the discharge of that compartment. The sum of these partial discharges is the total flood-discharge.

Variation of Velocity at a Point.—Opportunity was taken during the gauging described above to get a continuous series of observations of velocity on Vertical II., in order to ascertain what the local variation of velocity amounted to, and what number of revolutions must be observed to get sensibly constant mean values of the velocity. The readings were taken at 0·5 metre, at 3 metres, and at 6 metres from the surface, the last position being 0·3 metre above the bed. The meter being fixed, the time of each 100 revolutions was noted by counting seconds while observing a watch with long seconds hand. The actual times, and not the intervals, were noted, so that the agreement or disagreement of the results was not known during the observations. In this way 60 successive velocities were taken at 0·5 metre depth; 37 at 3 metres, and 22 at the bottom. Fig. 8 shows these results plotted, the times being taken as abscisses and the velocities as ordinates. The variability of the velocity at a point is very obvious, and though the mode of noting the time was somewhat rough, the differences are too great to be due to errors of observation.

The time-intervals for the two upper curves were about twelve seconds and for the bottom curve twenty seconds.

If now the velocities are calculated for 500 revolutions of the meter instead of for 100, a much more regular curve results, because the variations of velocity being periodic are better averaged in that longer period. Fig. 9 shows the curve for 0·5 metre depth calculated for each 500 revolutions of the meter, and it will be seen that the variation of velocity in periods of about one minute is very small. The following Table giving a part of the results shows the differences of velocity with 100-revolution observations and the comparative constancy with 500-revolution observations very clearly. It may be noted that in 100 revolutions about 22 metres, and in 500 revolutions about 110 metres, of water passed the current-meter.

Successive velocities in metres per second.			
At 0·5m. depth.		At 3m. depth.	At 6m. depth.
100 revolutions.	500 revolutions.	100 revolutions.	100 revolutions.
1·909	1·877	1·861	1·111
1·942	1·846	2·030	1·069
1·987	1·846	1·942	0·977
1·942	1·846	2·030	1·180
1·861	1·832	2·030	1·044
1·861	1·832	2·030	0·937
1·787	1·823	1·909	1·021
1·861	1·787	1·909	0·998
1·861	1·809	1·861	0·977
1·861	1·787	2·030	1·094
1·942	1·861	2·125	0·900
1·760	1·823	2·030	1·034
1·831	..	2·086	1·021
1·719	..	1·994	1·180
1·942	..	2·086	1·079
1·831	..	1·909	1·472

The Paper is accompanied by several diagrams, from which Plate 6 has been prepared.

(*Paper No. 1904.*)

"On the Horizontal Thrust of a Mass of Sand."

By GEORGE HOWARD DARWIN, M.A., F.R.S.,

Formerly Fellow of Trinity College, Cambridge.

1. ACCOUNT OF EXPERIMENTS.

THE pressure of loose earth against revetment-walls is a subject which is frequently being brought in a practical manner under the notice of engineers. A considerable number of theoretical investigations have been published by Coulomb, Rankine, Lévy, Boussinesq, and others, on this subject, but it appears that there is a singular deficiency of experimental data for testing the accuracy of the results of theory. The following Paper contains an account of some experiments made with that view in the summer of 1877. Circumstances prevented the Author from carrying out his original intention of experimenting with various forms of apparatus and different materials. Although, then, the investigation is somewhat incomplete, yet it seemed to have been carried far enough to establish several propositions of interest with regard to the horizontal thrust of sand. As will be seen below, the nature of those conclusions was such that the renewal of the experiments seemed inexpedient. An important Paper by Mr. Benjamin Baker,[1] M. Inst. C.E., on the lateral pressure of earthwork, was read before the Institution of Civil Engineers in 1881, and excited a discussion of much interest. That Paper, which has only recently been brought under the Author's notice, afforded the immediate inducement for bringing forward his results.

It is certain that, unless the theory agrees well with the facts, when the subject of experiment is of the finest and most uniform material, it will be very unlikely to give good results when the material is of the kind actually occurring in embankments. Several specimens were accordingly obtained of the sand which is used in sawing marble, and, as the apparatus was to be on a small

---

[1] "The Actual Lateral Pressure of Earthwork." Minutes of Proceedings Inst. C.E., 1880–81, vol. lxv., p. 140.

scale, the finest grained of the samples was chosen. It was the carefully washed and sieved road-scrapings from a flinty country, and consisted almost entirely of fine fragments of flint. When dried it formed a fine powdery sand with no large fragments in it. According to the received theory, the mechanical properties of a granular substance are completely determined when its specific gravity, and its angle of repose, are known, that is to say the greatest inclination to the horizon at which a talus will stand.

The angle of repose of the sand was determined by several experiments to be 35°, but this did not appear to be a constant which it was possible to determine within less than 1°, because in forming a talus there occurs some automatic sorting of the sand into finer and coarser particles. By the weighing of a vessel, into which the sand was poured as lightly as possible, the specific gravity in that condition was determined as 1·40. But when the sand was jarred, shaken, and thoroughly stirred up with a stick, the specific gravity rose to 1·55.

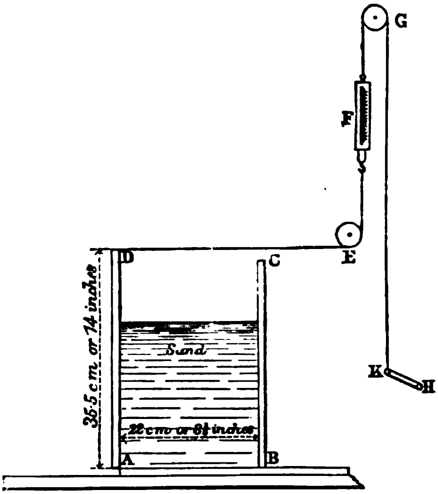
The effect of the stirring was peculiar. When the stick was first introduced, it easily penetrated to the bottom of the vessel, but after a little stirring the substance could be felt to stiffen at the bottom, and the stiffening then crept upwards, so that after a time the sand became impenetrable to the stick. This is not very surprising, seeing that after stirring the sand occupied a tenth less volume than before.

Here already there is a certain deficiency in the mathematical theories, since this very large variability of specific gravity is not taken into account; and whilst actual embankments no doubt correspond much more nearly to the sand in close order than in open order, the angle of repose of shaken sand is a phrase without a meaning. When the Author was beginning these experiments he had the advantage of discussing the subject with the late Professor Clerk Maxwell, who remarked that he supposed that the "historical element" would enter largely into the nature of the limiting equilibrium of sand. By this he meant that sand when put together in different ways would exercise different thrusts, although presenting visibly the same external appearance. The Author kept this valuable remark before him throughout, and found that Maxwell's conjecture was correct. The historical element is one which essentially eludes mathematical treatment.

For the purpose of experimenting a box was made, one end of which was a door turning on horizontal hinges at the bottom; the box was to be filled with sand, and it was then proposed to determine the least force capable of sustaining the door in position. Fig. A is a diagram of the apparatus in section. The supports for the various pulleys and accessories are omitted for the sake of simplicity.

The box A B C D is of the dimensions shown in the figure in centimetres and inches, and the width perpendicular to the section is 30·5 centimetres, or a foot, inside measure. The side B C was made to slide in and out, in order to facilitate the operation of filling the box with sand. It was originally more remote from A D, but the operation of filling the box was found so laborious that it was moved up closer, to the distance shown. The side B C had a film of sand glued to its surface. The two vertical sides parallel to the section (which are not, of course, shown) were prolonged for some way beyond A. The side A D, which is here called the door, turns on hinges at A, and is about 30 centimetres broad. The bottom of the box A B is of double thickness as far as A, to admit of the attachment of the hinges at A. A strip of soft silk was glued over the hinges A, to prevent their becoming clogged with sand. It is obvious that the door turned between a pair of vertical walls. As the door was to be easily movable, its edges could not be in absolute contact with the vertical sides of the box, and thus there were necessarily small cracks by which the sand might escape, or perhaps cause the door to jam against the sides.

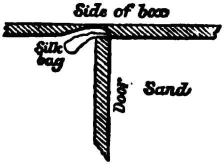
FIG. A.



Scale $\frac{1}{12}$ inch to 1 inch.

After some trouble this difficulty was overcome in a manner which seemed quite satisfactory. Each edge of the door would have left a crack of something less than $\frac{1}{4}$ millimetre open between it and the vertical side of the box, had the sides been flat all along. Fig. B exhibits one edge of the door and part of the vertical side of the box, as seen vertically from above when the door is upright. The flat side of the box is shown as scalloped out with a vertical groove running parallel with the edge of the door; in consequence of this arrangement the crack referred to was narrow when the door was upright, but became broader the moment the door yielded from its upright position. A narrow triangular piece of soft silk was glued by one of its edges along the inner face of the door, and along its other edge to the flat side of the box. When the door was upright, as shown in Fig. B, the silk formed a conical tube, marked as "silk bag," lying in the groove just outside of the door; and in the upright position of the door the silk just stopped up the narrow cracks, and made the box practically sand-tight. The breadth of the silk was such as to allow the top of the door to move outwards through 2 inches, when the silk became stretched tight and of course allowed no further displacement. There was a pair of stops fixed to the sides of the box in such a position that the door could not turn inwards past the vertical position. There was also a counterpoise attached to the top of the door, but not shown in Fig. A, and so arranged that the door was just in equilibrium when it was resting very lightly against these stops. Besides these stops there was a pair of bolts by which the door could be fixed in any position very nearly vertical, during the operation of filling the box with sand. When sand was poured into the box it was rare that any escaped into the silk bags, and the light contact of the silk with the sides of the box was found to cause no sensible friction. On the whole this method of making the box sand-tight, and at the same time avoiding the friction of the door against the sides, was found to be far more satisfactory than was expected, and it may be accepted that the results have not been sensibly vitiated by friction. And even if there were this cause of error, the greater part of the conclusions would be unaffected by it. The inner face of the door had a thin coating of sand glued to it, so that the friction of the sand against

FIG. B.



the door might be equal to the internal friction of the loose sand. The door was also graduated with a scale of centimetres, for the purpose of reading off the depth of the sand.

The box having been now described, the method must be explained for recording the couple acting on the door, when the sand was just on the point of slipping.

To the centre of the top of the door there was hooked on a silk cord DE (Fig. A), which ran horizontally to the pulley E, and then vertically to the spring-balance F—one of Salter's graduated on the metric system. From the upper end of the balance F there ran another cord which passed over the pulley G, and then ran vertically down to the spindle K, round which it was wound several times. The spindle K was formed of stout brass wire, and H was a crank-handle, by means of which the silk cord could be wound upon or unwound from the spindle K. On unwinding the silk slowly from the spindle, the tension on D E could be gradually relaxed, and the tension could be noted at each instant by watching the index of the balance.

The method of observation was to place the box flat on the floor; bolt the door in the vertical position; fill the box carefully, in the required manner, to any required depth; hook on the cord to the middle of the door; raise the tension of the cord to such an amount as would certainly hold the door in position; gently unbolt the door, and then gradually relax the tension by unwinding the spindle, whilst attentively watching the index of the spring-balance. When the door yielded there was a sudden motion of the index, and the position from which it yielded was noted down. The Author also found after some time that, by attentive listening, he could detect a yielding of the sand, so small as scarcely to move the index perceptibly; the ear was thus made to confirm the eye in the observations.

Suppose now that the box has been filled with sand of specific gravity w to a depth l, and that at the moment at which the tension of the cord is relaxed down to the point at which the door yields, that tension is T grams. At the instant of yielding, the couple tending to hold up the door is $T \times 35 \cdot 5$ gram-centimetres.

Then let L be the couple required to hold up a strip of the door of 1 centimetre in breadth, on the supposition that the box is infinitely wide, and let b be the effective breadth of the box. Then clearly $L b = T \times 35 \cdot 5$.

There is reason to believe, from the experiments of Series VII. below, that the sides of the box exercised but little influence in supporting the mass of sand, and for the reasons assigned below b

may be taken as 29 centimetres, being 1 centimetre less than the actual width of the door.

Let ϕ be the angle of repose of the sand.

Then, according to Rankine's formula—

$$T \times 35\cdot 5 = L b = \tfrac{1}{6}\, w l^3 b \tan^2 (\tfrac{1}{4}\pi - \tfrac{1}{2}\phi).$$

And according to Boussinesq's formula, given in the discussion on Mr. Baker's Paper, before referred to, viz., equation (16), with b zero, because the wall turns about the lower edge in contact with the mass of sand, and with $\phi_1 = \phi$, because the friction against the wall is equal to the internal friction of the sand—

$$T \times 35\cdot 5 = L b = \tfrac{1}{6}\, w l^3 b \tan^2 (\tfrac{1}{4}\pi - \tfrac{1}{2}\phi) \frac{\cos (\tfrac{1}{4}\pi - \tfrac{1}{2}\phi)}{\cos (\tfrac{3}{2}\phi - \tfrac{1}{4}\pi)} \cos \phi.$$

These formulas will be considered below in detail, but at present it is only necessary to state that one important object of the experiments is to test their truth. At the time, indeed, of making the experiments the Author was not aware of Boussinesq's formula, and only made the comparison with Rankine's. It was intended to test the following points, viz., whether the oversetting couple L varies as the cube of the depth of the sand, and whether the constant introduced in order to express the actual amount of that couple corresponds in magnitude with either of the theoretical functions of the angle of repose of the sand. Experiments were also to be undertaken to find the couple L when the surface of the sand was not horizontal; the theoretical formulas for these cases will be given in the second part of this Paper. Lastly, it was necessary to determine what amount of supporting influence was exercised by the sides of the box.

It will be seen below, in discussing the experiments, that these questions are scarcely susceptible of perfectly rigorous answers; but at present it is only necessary to explain the method by which the answers, as far as they are given, are to be extracted from the experimental results.

The graphical method of record was found to be well suited for the purpose; for the record of a great number of experiments could be made on a single figure, and the result to be deduced could be most easily extracted from the figure itself. The records will accordingly be here reproduced in the graphical form.

The horizontal axis is taken to indicate the depth l of the sand (Figs. 1–7 below), the numbers along the l-axis being centimetres of depth from the bottom of the box. The vertical ordinates represent T, the tension of the cord in grams at the moment of slipping.

The graphical records are all drawn to the following scale:—The abscisses, representing the centimetres of depth (l) of the sand, are drawn to the scale of $\frac{1}{8}$ inch to the centimetre, and the numbers represent centimetres. The ordinates are drawn to the scale of $\frac{2}{3}$ centimetre to 100 grams. In each Figure there is introduced a little diagram, to explain the method in which the box was filled with sand; that is to say, to record "the historical element." There are also drawn one or more cubical parabolas, of which the equation referred to the coordinate-axes is $T = \mu l^3$; the value of μ corresponding to each parabola is given. Then by mere inspection it was easy to see which parabola, if any, best agreed with the observations, and by a little simple arithmetic the numerical value of the function of the effective breadth of the door, and of the angle of repose of the sand, could be evaluated.

In fact, each set of the experiments gave the value of $L b \div \frac{1}{6} w l^3$, which for the series I. to IV. should, according to Rankine, be equal to $b \tan^2 (\frac{1}{4} \pi - \frac{1}{2} \phi)$, and according to Boussinesq should be $b \tan^2 (\frac{1}{4} \pi - \frac{1}{2} \phi) \cos (\frac{1}{4} \pi - \frac{1}{2} \phi) \sec (\frac{3}{2} \phi - \frac{1}{4} \pi) \cos \phi$.

On dividing these values by b (which is taken as 29 centimetres) the numerical values of the function of ϕ are obtained.

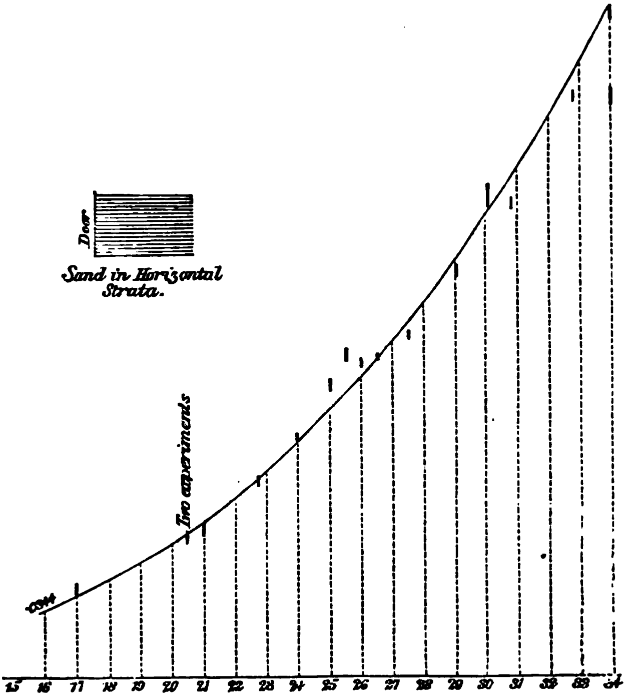
After these explanations, the records of the experiments themselves may be given.

It may be worth mentioning that there was at first an index attached to the door, by which any displacement of the door was much magnified. In series I. and II. this index was still in use, but it was ultimately dispensed with, after some practice had shown it to be unnecessary.

Series I., Fig. 1.—The box was filled with sand with a shovel, or with a tin canister, and at each stage in the filling the strata were maintained nearly level, but it was not possible to make the strata truly level in this way.

The most prominent feature in this series of experiments, recorded in Fig. 1, was the difficulty of asserting that the sand slipped at any definite time. As soon as the tension T had fallen to within 100 or 200 grams of the point at which equilibrium quite broke down, it was observable that there was an almost continuous yielding of the door as the tension was gradually relaxed. After the upper edge of the door had yielded by perhaps $\frac{1}{20}$ inch the motion began to be much more marked, and the diminution of tension was accompanied by obvious motions of the door, which yielded by little jerks. It was this marked yielding which was taken as the phenomenon to be observed, and it is accordingly only possible to state that this took place with approximately a certain tension on the cord. This fact is noted in Fig. 1 by the record of each observation consisting of a vertical line of greater or less length, according as the jerking motion of the door ranged through a greater or less variation of tension. The curve inserted in the figure is the cubical parabola which, as far as may be judged, best satisfies the observations. Considering how much the element of judgment entered in this record, the experiments seem to accord tolerably well with the law that the oversetting couple varies as the cube of the depth of the sand.

FIG. 1.



This parabola shows that—

$$T = 0{\cdot}0344\, l^3 \text{ gram}$$

$$Lb = 1{\cdot}221\, l^3 \text{ gram-centimetre}$$

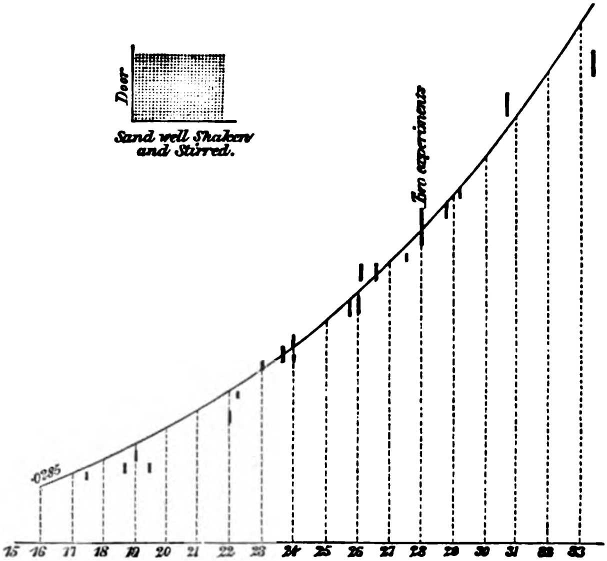
$$= \tfrac{1}{6}\, w l^3 \times 5{\cdot}234, \text{ with } w = 1{\cdot}40$$

and $$L = 0{\cdot}180 \times \tfrac{1}{6}\, w l^3 \text{ gram-centimetre}$$

with $$b = 29 \text{ centimetres, and } w = 1{\cdot}40$$

Series II., Fig. 2.—As it was found that there was no sensible efflux of sand into the silk bags, and that the door had no tendency to jam after it had yielded by a small distance, the Author attempted to combine two sets of observations with only one filling of the box. Accordingly, after the tension at which the jerky yielding became prominent had been determined, the door was bolted in the position in which it happened to be, that is to say, in all cases exceedingly little out of the vertical. The sand was then thoroughly stirred up with a stick, and the sides of the box hammered. This caused the sand to pack closer by fully 10 per cent., and the level in the box to fall by nearly a centimetre. The door was unbolted and a fresh observation was made. The difficulty of determining what was the tension of the cord, when limiting equilibrium was attained, was considerably greater than in Series I. Nevertheless, observations were taken of the tensions at which the yielding by jerks first became clearly marked, and the results are recorded in Fig. 2. It seems probable that some part of the difficulty arose from the door being slightly elastic, and being put into a state of strain by the ramming

Fig. 2.



down of the sand. This strain would be gradually relaxed as the tension on the cord diminished.

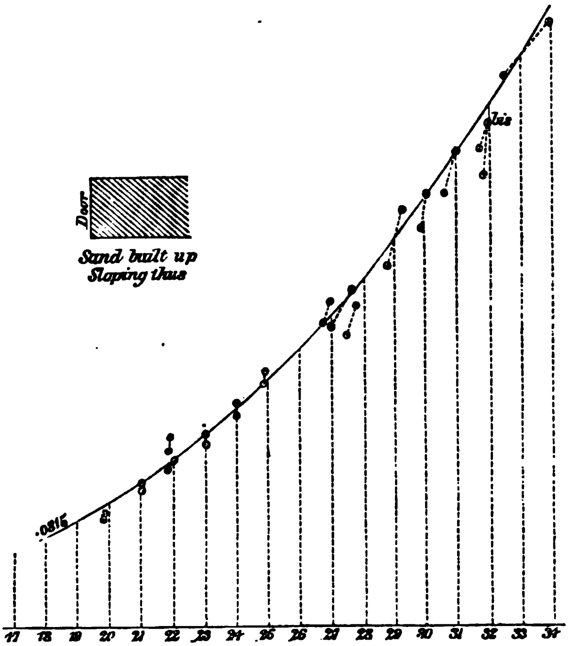
The cubical parabola which seems best to fit the facts is such that—

$$\begin{aligned} T &= 0{\cdot}0285\, l^3 \\ Lb &= 1{\cdot}0118\, l^3 \\ &= 3{\cdot}916 \times \tfrac{1}{6}\, w l^3, \text{ with } w = 1{\cdot}55 \\ \text{and} \quad L &= 0{\cdot}132 \times \tfrac{1}{6}\, w l^3, \text{ with } w = 1{\cdot}55,\ b = 29. \end{aligned}$$

This series of experiments cannot be regarded as satisfactory, but as it is the only one in which the earth was stirred and shaken, it is given for what it is worth.

Series III., Fig. 3.—In this series the sand was poured in at the side of the box next to the door, so that at each stage of filling it lay at the angle of repose, with the uncovered portion of the door making an obtuse angle with the talus. When the sand was filled

FIG. 3.

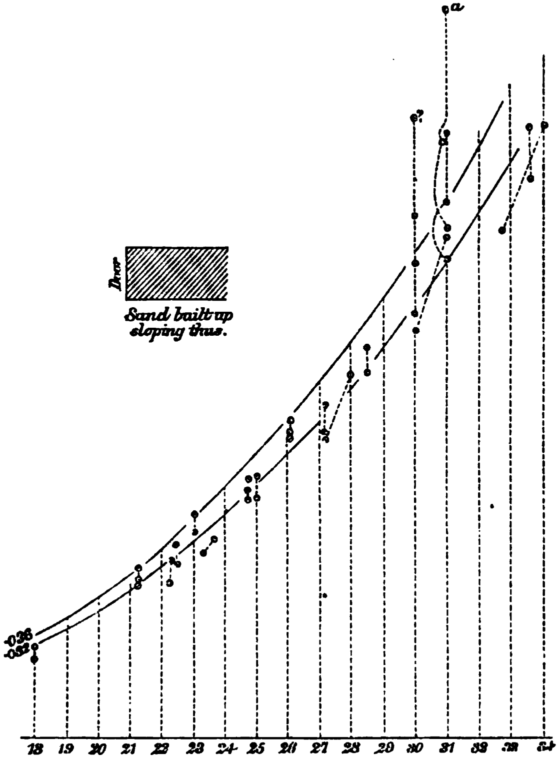


up to the desired height the part of the box lying over the talus was filled in, the strata being kept parallel to the talus, until an approximately level surface to the mass of sand was formed. The surface was finally levelled by scraping it with a straight-edge of wood. Judging by the experience gained in the first two series, the Author fully expected to observe a similar mode of break down of equilibrium. He was therefore surprised, in making the first experiment, not to note any perceptible preliminary yielding before the equilibrium completely broke down, and the door yielded to the full extent permitted to it by the silk bags. Indeed, the record of this first experiment was lost through surprise. In subsequent experiments the breakdown of equilibrium was nearly always very marked, although not generally so complete as in the first experiment. After the first sand-slip the tension of the cord was further relaxed until a second slip took place. It will be noted in Fig. 3 that the observations are recorded by two small circles joined by dotted lines. The centre of the upper circle records the tension on the cord when the first slip took place; the lower records the second slip. If the first slip was large the sand fell considerably in the box, so that the second slip corresponds to a smaller depth of sand; thus the inclination of the joining line between a pair of marks gives an indication of the magnitude of the first slip. For instance, when the sand was at first 34 centimetres deep the first slip was a very large one. The parabola drawn through the observations indicates that

$$
\begin{aligned}
T &= 0{\cdot}0315\, l^3 \\
L\, b &= 1{\cdot}1183\, l^3 \\
&= 4{\cdot}793 \times \tfrac{1}{8}\, w\, l^3,\ \text{with } w = 1{\cdot}40 \\
\text{and} \qquad L &= 0{\cdot}165 \times \tfrac{1}{8}\, w\, l^3,\ \text{with } w = 1{\cdot}40,\ b = 29.
\end{aligned}
$$

Series IV., Figs. 4, i, 4, ii, 4, iii.—In this series the sand was again poured into the box, so that at each stage of the filling it stood at the angle of repose. But the talus was now made to slope the opposite way, so that the uncovered portion of the door made an acute angle with the talus. The sand was filled up approximately to the level, and smoothed with a piece of wood as before. The phenomena were quite different, and somewhat capricious. As in Series III., the successive slips are indicated by small circles joined by dotted lines. In general the first slip was excessively small, and it was sometimes not easy to decide whether the sand had slipped or not, but the question was not unfrequently decided by a very gentle hissing noise, which could often be noted. Then it required a large relaxation of tension before there was a second small slip, and occasionally three or four successive slips were noted in this way. On the other hand, the equilibrium would occasionally break down suddenly by a large slip, as in Series III. The record of these experiments is given in Fig. 4, i. In order that the figure may not be confused the dotted line joining some of the pairs of dots is drawn curved. There are notes added with regard to some of the experiments. The results

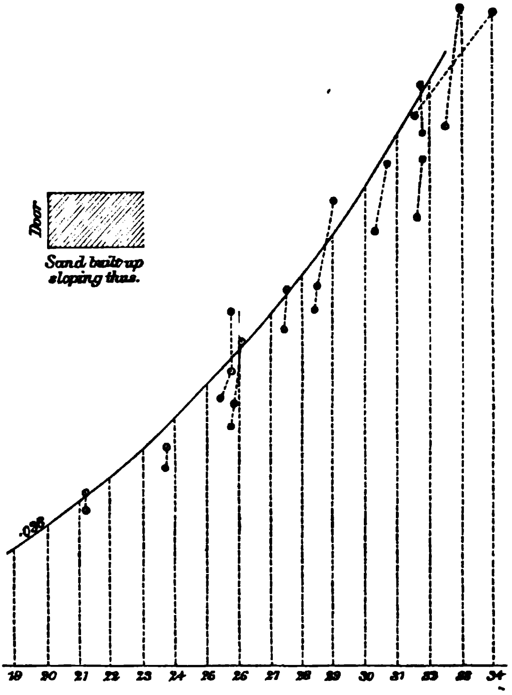
FIG. 4, i.



a An excessively small slip.
? Some doubt as to whether there was really a slip.

are so irregular that it is hardly possible to say which cubical parabola suits them best. The parabola $T = 0{\cdot}036 \times l^3$ seems as near as any of them, but some of the observations are very discordant from it. This would give $L = 0{\cdot}189 \times \frac{1}{6} wl^3$, when $w = 1{\cdot}40$, $b = 29$. A new series of experiments (Fig. 4, ii) of the same kind was then begun, especially close attention being paid to the first small slips, and to the manner of filling the box, so that it should not be jolted at all before the experiment began. A similar capriciousness is observable in the results, and the cubical parabola which suits the observations perhaps better than any other is $T = 0{\cdot}036 \times l^3$. In Fig. 4, iii are collected together all the first slips of both the above series. The observations marked B were large slips, those marked *b* were moderately large, and those marked ? were somewhat doubtful on account of the excessive smallness of the slip. It will be seen that no parabola really

FIG. 4, ii.

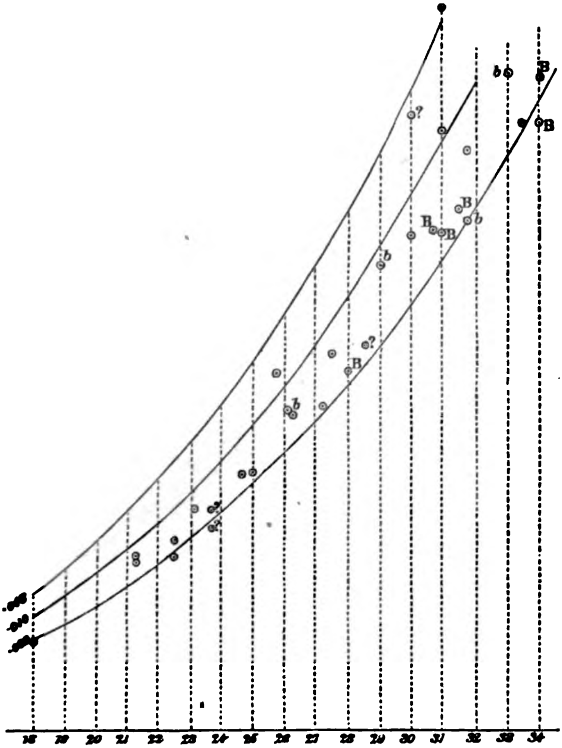


satisfies these observations well, but they group themselves approximately about

$$T = 0\cdot036 \times l^3$$
$$L b = 1\cdot278 \times l^3$$
$$= 5\cdot477 \times \tfrac{1}{6}\, w\, l^3, \text{ with } w = 1\cdot40$$

and $$L = 0\cdot189 \times \tfrac{1}{6}\, w\, l^3, \text{ with } w = 1\cdot40,\ b = 29.$$

FIG. 4, iii.



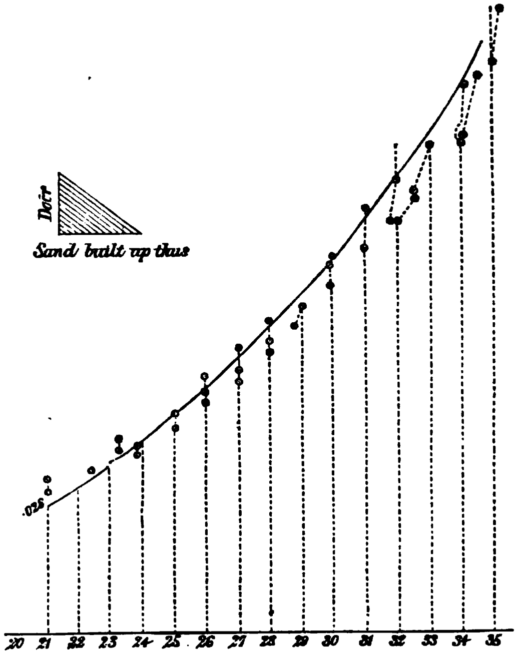
In cases marked ? there was some doubt, generally because of the smallness of the slip. The cases marked B were large slips, those b were moderate.

Series V., Fig. 5.—This relates to a different class of experiment, namely, when the surface of the sand is no longer level, but stands at the angle of repose.

In this series the talus of sand made an obtuse angle with the uncovered portion of the door, so that the grains of sand tended to roll away from the door. Throughout the operation of filling the box this arrangement of strata was maintained. The depth l of the sand in this case means the depth at the movable door.

The observations are recorded in the same way as before, and

Fig. 5.



are very fairly consistent amongst themselves. The parabola shows

$$T = 0{\cdot}028 \times l^3$$

$$Lb = 0{\cdot}994 \times l^3$$

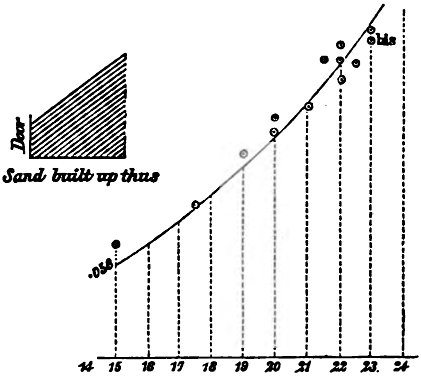
$$= 4{\cdot}26 \times \tfrac{1}{6}\, w\, l^3,\ \text{with } w = 1{\cdot}40$$

and $$L = 0{\cdot}147 \times \tfrac{1}{6}\, w\, l^3,\ \text{with } w = 1{\cdot}40,\ b = 29.$$

The couple is, of course, notably less than when the sand was level.

Series VI., Fig. 6.—In this series the surface of the sand was not level, but stood at the angle of repose. It differed from Series V. in that the talus made an acute angle with the uncovered portion of the door. Throughout the operation of filling the box this arrangement of strata was maintained. As the sand stood higher at the side of the box remote from the door, it was not possible for the level of the sand to stand higher than 23 centimetres at the door. Fig. 6 gives the record. The first slips only are entered in this figure. The observations were on this occasion reduced arithmetically, but the result of the reduction is introduced graphically in the cubical parabola shown in Fig. 6. The result is that

FIG. 6.



$$\begin{aligned} T &= 0{\cdot}0555 \times l^3 \\ L b &= 1{\cdot}970 \times l^3 \\ &= 8{\cdot}44 \times \tfrac{1}{6} w l^3, \text{ with } w = 1{\cdot}40 \end{aligned}$$

and $$L = 0{\cdot}291 \times \tfrac{1}{6} w l^3, \text{ with } w = 1{\cdot}40 \text{ and } b = 29.$$

The couple is almost exactly twice as great as when the talus sloped the other way in Series V.

Series VII., Fig. 7, i, ii, iii.—These observations were undertaken with the view of determining what supporting influence was exerted by the sides of the box, and thus to evaluate the effective width of the box.

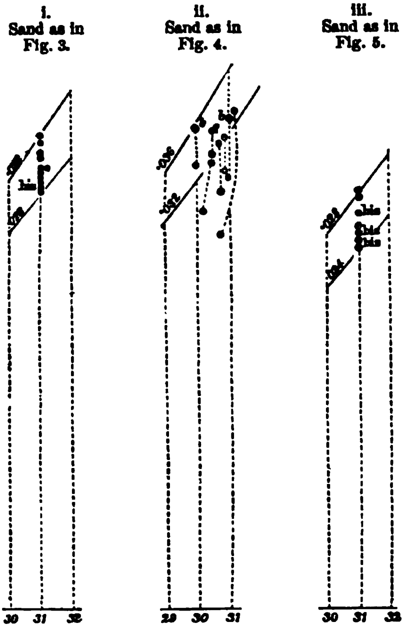
For this purpose a partition was fixed in the box dividing it, perpendicular to the door, into two equal parts. The partition came quite up to the door, which it just touched when the door was vertical.

As it seemed that the law of the cube of the depth was sufficiently well established — or, at least, that that law was more nearly in accordance with the facts than any other—observations were only taken with the sand at approximately one depth, viz., 31 centimetres.

In the first set of experiments with the partition (Fig. 7, i) the sand was built up as in Series III. The results were reduced arithmetically, but are recorded graphically. They give

$$T = 0{\cdot}0296\, l^3$$
$$Lb = 1{\cdot}049\, l^3 = 4{\cdot}50 \times \tfrac{1}{6}\, w l^3, \text{ when } w = 1{\cdot}40$$
and $$L = 0{\cdot}164 \times \tfrac{1}{6}\, w l^3, \text{ when } w = 1{\cdot}40,\ b = 27\tfrac{1}{2}$$
Series III. had given $L = 0{\cdot}165 \times \frac{1}{6}\, w l^3$, when $b = 29$.

FIG. 7.


i. Sand as in Fig. 3.

ii. Sand as in Fig. 4.

iii. Sand as in Fig. 5.

In the next set the sand was built up as in Series IV. The results were reduced arithmetically, but are recorded graphically in Fig. 7, ii. They give

$$T = 0{\cdot}0328\, l^3$$
$$Lb = 1{\cdot}165\, l^3 = 4{\cdot}99 \times \tfrac{1}{6}\, w l^3, \text{ when } w = 1{\cdot}40$$
and $$L = 0{\cdot}182 \times \tfrac{1}{6}\, w l^3, \text{ when } w = 1{\cdot}40,\ b = 27\tfrac{1}{2}$$
Series IV. had given $L = 0{\cdot}189 \times \frac{1}{6}\, w l^3$, when $b = 29$.

In the next set the sand was built up as in Series V. The results were reduced arithmetically, but are recorded graphically in Fig. 7, iii. They give

$$T = 0\cdot0261\,l^3$$
$$L\,b = 0\cdot926\,l^3 = 3\cdot967 \times \tfrac{1}{6}\,w\,l^3, \text{ when } w = 1\cdot40$$
and $$L = 0\cdot144 \times \tfrac{1}{6}\,w\,l^3, \text{ when } w = 1\cdot40,\ b \doteq 27\tfrac{1}{2}$$
Series V. had given $L = 0\cdot147 \times \frac{1}{6}\,w\,l^3$, when $b = 29$.

Thus the twenty-nine experiments with the partition gave sensibly the same results as when the partition was absent, provided b (the effective breadth of the box) is taken as 27½ centimetres instead of 29 centimetres.

The actual thickness of the partition was 1·3 centimetre, and therefore the actual diminution of the width of the box was 1·3 centimetre. But when the partition was in, there were four vertical surfaces tending to support the mass of sand, instead of only two, as there were when the partition was absent.[1] If, therefore, these vertical surfaces had exercised much supporting influence, the experiments of Series VII. would not have given results in accordance with those of the preceding series, without a diminution of the effective breadth of the box over and above the actual diminution due to the thickness of the partition. As a diminution of b nearly equal to the thickness of the board makes the two sets of experiments accord admirably with one another, it may safely be concluded that the vertical sides of the box exercised very little supporting effect on the sand.

In reducing all the previous experiments, a centimetre has been allowed for this diminution of breadth, and b has always been taken as 29 centimetres, instead of 30 centimetres.

The total number of experiments recorded in Figs. 1–7, and included in the graphical analysis is 143.

The experiments of Series I. and II. are probably somewhat less satisfactory than the later ones, because after much experience the

---

[1] If the experiments had been accurate enough to admit of rigorous treatment, the following would have been the process. The actual width of the door is 30 centimetres. Let x be the diminution of width due to the support afforded by two sides, as in Series I.–VI. Then $2x$ is the diminution of breadth due to the support of four sides, as in Series VII. In Series VII. the breadth of door in actual contact with the sand was $30 - 1\cdot3 = 28\cdot7$ centimetres. Thus, comparing III. with VII, i, in order that the two sets of experiments may give identical results, the equation $\frac{4\cdot793}{30 - x} = \frac{4\cdot493}{28\cdot7 - 2x}$ must be satisfied. This gives $x = 0\cdot65$. Similarly, the comparisons of IV. with VII, ii, and of V. with VII, iii, give respectively $x = 1\cdot24$, and $x = 0\cdot70$. It may thus be concluded that the diminution of breadth of the box due to lateral support must be about a centimetre, and accordingly b has been taken as 29 in the reduction of the experiments of Series I. to VI.

Author became more alive to the phenomena to be watched. But before making any of the experiments which are here analysed, he had already made over seventy experiments which were all rejected for defects in the apparatus. Moreover, in each series several experiments are excluded on account of some accidental jolt or other circumstance, which might have vitiated the result.

After completing the work of which an account has now been given, the Author was interrupted by other occupations, and has not resumed the subject.

2. DISCUSSION OF THE EXPERIMENTS.

The results of the experiments must now be compared with the theoretical formulas. In the Series I. to IV. the surface of the sand was flat. According to Rankine the couple per unit breadth of the door should be expressed by

$$L = \tfrac{1}{6}\, w\, l^3 \tan^2 (\tfrac{1}{4}\pi - \tfrac{1}{2}\phi),$$

and according to Boussinesq by

$$L = \tfrac{1}{6}\, w\, l^3 \tan^2 (\tfrac{1}{4}\pi - \tfrac{1}{2}\phi) \frac{\cos (\frac{1}{4}\pi - \frac{1}{2}\phi)}{\cos (\frac{3}{2}\phi - \frac{1}{4}\pi)} \cos\phi.$$

The angle of repose of the sand was determined, as nearly as might be, at 35°. In order to show the influence of error in this determination it may be stated that when $\phi = 34°, 35°, 36°$

$$\tan^2 (\tfrac{1}{4}\pi - \tfrac{1}{2}\phi) = 0\cdot283,\ 0\cdot271,\ 0\cdot260 \text{ respectively,}$$

and

$$\tan^2 (\tfrac{1}{4}\pi - \tfrac{1}{2}\phi) \frac{\cos (\frac{1}{4}\pi - \frac{1}{2}\phi)}{\cos (\frac{3}{2}\phi - \frac{1}{4}\pi)} \cos\phi = 0\cdot208,\ 0\cdot199,\ 0\cdot189$$

respectively.

Thus the two theories should give, for the values of the coefficient, for this sand, about 0·27 and 0·20 respectively. Now the following were found as the values of the coefficient by experiment: from Series I. 0·180, from Series II. 0·132, from Series III. 0·165, from Series IV. 0·189. The discrepancy from Rankine's value is enormous, and that theory may be safely neglected. The difference from Boussinesq's value in three out of four of the series is not large, but it is (except in IV.) larger than can be fairly put down to errors of observation and an erroneous estimate of the effective width of the box. The disagreement of the various series amongst themselves is considerable, and in Series IV. especially there was much uncertainty as to the proper value to be taken. It will be pointed out below in what way it appears likely that the *à priori* assumptions of the theories are deficient.

The experiments of Series V. and VI. in which the sand formed a talus at the angle of repose will now be considered. It appears from Mr. Boussinesq's Paper[1] that, in the case of the ascending talus (Series VI.) supported by a vertical wall, the formula for the oversetting couple is

$$L = \tfrac{1}{6}\, w\, l^3 \cos^2 \phi.$$

Mr. Boussinesq has not been able to evaluate a formula for the descending talus, which formed the subject of experiment in Series V.

---

[1] The memoir is entitled "Essai théorique sur l'equilibre d'élasticité des massifs pulvérulents, &c. Mémoire présenté à la classe des sciences dans la séance du 6 Juin 1874." Société Royale des Sciences de Belgique. It has also been published by Gautier Villars of Paris.

On p. 93 (§ 41 of Sec. VIII.) it is stated that the moment of the thrust about the exterior base of the wall is

$$\tfrac{1}{2}\, \rho\, g\, h^2\, K\left(\tfrac{1}{3}\, h \cos \omega - b \sin \omega\right).$$

Here his $\rho\, g$ is w, the density of the sand; his h is l, the depth of sand against the wall; b is the thickness of the wall, which must be taken as zero; ω is the inclination of the talus to the horizon, estimated as positive when the talus makes an acute angle with the uncovered portion of the wall; and K a certain coefficient. Thus so far his result is in the notation of the present Paper $\tfrac{1}{6}\, w\, l^3\, K \cos \omega$.

On p. 126 (§ 47 of Sec. IX.) it will be found that when the sand is on the point of slipping (*état ébouleux*),

$$K = \tan\left(\tfrac{1}{4}\pi - \tfrac{1}{2}\phi\right) \frac{\cos \psi \cos (\phi + \delta) \cos (\omega - i)}{\cos (\phi_1 - \delta) \cos (\omega + \psi)}.$$

Here ϕ is the angle of repose of the sand.

On p. 108 (§ 44) i is defined as the inclination of the sustaining wall to the vertical; in the present case i is of course zero.

On p. 109 ψ is defined by the equation

$$\sin (\omega + 2\,\psi) = \frac{\sin \omega}{\sin \phi}.$$

And on p. 126, it appears that

$$\delta = \tfrac{1}{4}\pi - \tfrac{1}{2}\phi - \psi - i,$$

where, as already remarked, i is to be put as zero.

The coefficient of friction between the wall and the sand is $\tan \phi_1$. And as in the above experiments the door was sprinkled with sand $\phi_1 = \phi$.

When the surface is level, by putting $\omega = o$, $i = o$, $\phi_1 = \phi$, the formula is obtained which has been quoted above for the case of the horizontal surface.

Next, where the surface forms a talus at the angle of repose, and where ω is positive and equal to ϕ, the formula quoted in the text is obtained.

Finally, where the surface forms a talus at the angle of repose, and where ω is

Rankine's theory, as applied to a talus, shows that the resultant action parallel to the line of greatest slope of the talus per unit of area across a vertical interface, whose plane is parallel to a horizontal line drawn across the talus, is expressed by

$$w\,z\cos\omega\,\frac{\cos\omega-\sqrt{(\cos^2\omega-\cos^2\phi)}}{\cos\omega+\sqrt{(\cos^2\omega-\cos^2\phi)}}.$$

Where z is the depth of the interface vertically below the surface, ω is the slope of the talus, and ϕ the angle of repose. He supposes that the action on a supporting wall is identical with that across an ideal surface cutting a complete talus. In the case where the talus has the slope ϕ, the force becomes simply $w\,z\cos\phi$, and the moment of all the forces about the base of the wall is

$$\int_o^l w\,z\cos^2\phi\,(l-z)\,d\,z.$$

Completing the integration there results, as before from Boussinesq,

$$L=\tfrac{1}{6}\,w\,l^3\cos^2\phi,$$

---

equal to $-\phi$, the formula for K gives the value zero. The formula in fact ceases to be even approximately true, and no other is investigated.

In a letter to the Author, dated January 6th, 1883, Mr. Boussinesq says (translation):—"In the memoir of 1874 I only attach importance, at least from the practical point of view, to § ix. The preceding paragraphs have only a theoretical value, because I had at that time allowed myself to be led into error by following Rankine, who, very erroneously, did not take into account the perturbations caused by the neighbourhood of the wall. . . . . My present ideas on the thrust of earth, from a practical point of view, are presented in the short note which you have seen in the discussion on Mr. Baker's Paper. . . . . [A new extended edition is published in the *Annales des Ponts et Chaussées.*] . . . I find the results of your useful observations, 0·180, 0·132, 0·165, 0·189, as concordant with my theoretical result 0·20, as could be expected (except perhaps the second); for the theoretical formula supposes the mass of earth and the wall infinitely long in the horizontal direction, and it neglects the disturbing effect of the solid soil which supports the mass of earth, treating the subject as though the depth of the earth were also infinite. To take into account the influence of the subjacent soil would complicate the problem inextricably. It must be observed, moreover, that the horizontal force which you had to measure must have been smaller than the horizontal component of the thrust itself, however little that force be aided by accessory friction in keeping the thrust (*sic*, query sand) in equilibrium. [The Author fails to comprehend this remark.] . . . . As far as relates to the case where ω is negative . . . . I do not think that one can rely on my formula (especially when $i=o$), because it is founded on the hypothesis that the angle δ is small. Now for negative values of ω, this angle δ soon becomes considerable, and it has begun to be even somewhat too large when $\omega=o$. . . . . The cases where δ is large, which are those where ω is negative, would necessitate integrations which appear to me to pass the power of analysis."

Since action and reaction across an ideal interface are equal and opposite, it is obvious that moments of the action and reaction about a point vertically under the interface are equal and opposite. If the interface be solidified into a wall, equal and opposite moments are got about the base of the wall, whether it be supposed that all the sand is removed from higher up than the wall, or lower down. Thus the two cases of the ascending and descending talus should give the same oversetting couple.

When $\phi = 35°$, $\cos^2 \phi = 0 \cdot 671$, and to accord with theory the experiments of Series V. and VI. should both, according to Rankine, and the latter only according to Boussinesq, have given

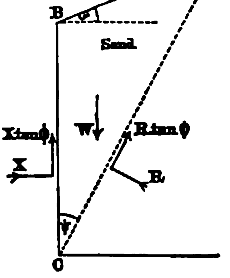
$$L = \tfrac{1}{6}\, w\, l^3 \times 0 \cdot 671.$$

Series V. gave, however, for the numerical factor 0·147, and Series VI. gave 0·291. This shows that the theory in this case is utterly at fault.

It will now be shown how, by means of some very simple assumptions with regard to the equilibrium of sand—assumptions which seem to have as good grounds for acceptance as the more complex views which form the basis of the theories of Rankine and Boussinesq—formulas can be arrived at which present some sort of concordance with the results of experiment. The Author was, in fact, surprised to find that the method suggested actually gives Boussinesq's formula for the case of the flat surface, and moreover gives formulas very fairly concordant with the results of the Series V. and VI.

Only the case of the vertical wall will be considered, for it is here alone that there is a datum for comparison. It will be clear, however, that the same principles might be applied in almost any case.

FIG. C.



It is first assumed that the centre of pressure of a flat vertical surface immersed in the sand obeys the hydrostatic law, and is at two-thirds of the depth below the surface. This is confirmed experimentally by the fact that the oversetting couple varies, with some degree of accuracy, as the cube of the depth. It is next assumed that a certain wedge of earth in the rear of the wall is simply supported by the pressures and frictions acting across the face of the wall and the internal face in the mass of sand. In Fig. C, let B C be the wall; A B the talus supported by it; let ω be the slope of the talus; and let B C $= l$ the depth of sand. Let A B C be the wedge which is assumed to be supported by the wall; and let the angle at C be called ψ. Let $\tan \phi$ be the coefficient of friction within the sand, and $\tan \phi_1$ that between the sand and the wall; w the density of the sand.

Then the wedge, whose weight is W, is supported by forces as indicated by arrows in the figure.

$$\text{The angle A} = \pi - (\tfrac{1}{2}\pi + \omega + \psi) = \tfrac{1}{2}\pi - (\omega + \psi).$$

$$\text{Then} \quad \text{A B} = l\frac{\sin \psi}{\cos(\omega + \psi)}, \quad \text{A C} = l\frac{\cos \omega}{\cos(\omega + \psi)},$$

$$\text{and} \quad W = \tfrac{1}{2} w \,.\, \text{A B} \,.\, \text{A C} \sin \text{A} = \tfrac{1}{2} w l^2 \frac{\sin \psi \cos \omega}{\cos(\omega + \psi)}.$$

Resolving horizontally and vertically,

$$X = R(\cos \psi - \tan \phi \sin \psi) = R\frac{\cos(\psi + \phi)}{\cos \phi},$$

$$X \tan \phi_1 + R(\sin \psi + \tan \phi \cos \psi) = W,$$

$$\text{or } X \tan \phi_1 + R\frac{\sin(\psi + \phi)}{\cos \phi} = \tfrac{1}{2} w l^2 \frac{\sin \psi \cos \omega}{\cos(\omega + \psi)}.$$

Substituting for X in terms of R,

$$R\{\cos(\psi + \phi)\tan \phi_1 + \sin(\psi + \phi)\} = \tfrac{1}{2} w l^2 \frac{\sin \psi \cos \omega \cos \phi}{\cos(\omega + \psi)}.$$

$$\left.\begin{aligned} \text{Whence } R &= \tfrac{1}{2} w l^2 \frac{\sin \psi \cos \omega \cos \phi \cos \phi_1}{\cos(\omega + \psi)\sin(\psi + \phi + \phi_1)} \\ \text{and} \quad X &= \tfrac{1}{2} w l^2 \frac{\sin \psi \cos \omega \cos(\psi + \phi)\cos \phi_1}{\cos(\omega + \psi)\sin(\psi + \phi + \phi_1)} \end{aligned}\right\}.$$

In order that the equation of moments about C may be satisfied, it is necessary that the forces R and $R \tan \phi$ should act at a certain point in the side A C, but it is unnecessary to determine the point for the purpose of finding the moment tending to overset the wall. Part of the assumption made is that the pressure X acts at two-thirds of the depth l. Hence the moment L tending to upset B C about C is given by

$$\begin{aligned} L &= \tfrac{1}{3} l X \\ &= \tfrac{1}{6} w l^3 \frac{\sin \psi \cos \omega \cos(\psi + \phi)\cos \phi_1}{\cos(\omega + \psi)\sin(\psi + \phi + \phi_1)}. \end{aligned}$$

The case of the smooth wall, in which $\phi_1 = 0$, will be passed over. When the friction against the wall is equal to the internal friction of the sand $\phi_1 = \phi$.

First, suppose the upper surface of the soil is level (Series I.–IV.), so that $\omega = 0$, and $\phi = \phi_1$.

Then

$$L = \tfrac{1}{6}\, w\, l^3 \tan\psi \frac{\cos(\psi + \phi)}{\sin(\psi + 2\phi)} \cos\phi,$$

Following Coulomb and others, and supposing the wedge to be bounded by a plane bisecting the angle between a free talus and the vertical, and assuming that ϕ is the angle of such a talus, $\psi = \frac{1}{4}\pi - \frac{1}{2}\phi$, it results that

$$L = \tfrac{1}{6}\, w\, l^3 \tan^2(\tfrac{1}{4}\pi - \tfrac{1}{2}\phi) \frac{\cos(\frac{1}{4}\pi - \frac{1}{2}\phi)}{\cos(\frac{3}{2}\phi - \frac{1}{4}\pi)} \cos\phi,$$

and this is Boussinesq's formula for this case. It has already been seen that it presents a fair approximation to some of the experimental results.

Secondly, suppose the upper surface stands at the angle of repose, and that $\omega = +\phi$ (Series VI.), then with $\phi = \phi_1$,

$$L = \tfrac{1}{6}\, w\, l^3 \sin\psi \frac{\cos^2\phi}{\sin(\psi + 2\phi)}.$$

If, as in the first case, $\psi = \frac{1}{4}\pi - \frac{1}{2}\phi$, this becomes

$$L = \tfrac{1}{6}\, w\, l^3 \sin(\tfrac{1}{4}\pi - \tfrac{1}{2}\phi) \frac{\cos^2\phi}{\cos(\frac{3}{2}\phi - \frac{1}{4}\pi)}.$$

Thirdly, suppose the upper surface stands at the angle of repose, but with the talus in the opposite direction, so that $\omega = -\phi$ (Series V.); then with $\phi = \phi_1$,

$$L = \tfrac{1}{6}\, w\, l^3 \sin\psi \frac{\cos^2\phi}{\cos(\psi + 2\phi)} \frac{\cos(\psi + \phi)}{\cos(\psi - \phi)}.$$

Putting $\psi = \frac{1}{4}\pi - \frac{1}{2}\phi$ as before, this becomes

$$L = \tfrac{1}{6}\, w\, l^3 \sin(\tfrac{1}{4}\pi - \tfrac{1}{2}\phi) \frac{\cos^2\phi}{\cos(\frac{3}{2}\phi - \frac{1}{4}\pi)} \frac{\sin(\frac{1}{4}\pi - \frac{1}{2}\phi)}{\cos(\frac{3}{2}\phi - \frac{1}{4}\pi)}$$

or

$$L = \tfrac{1}{6}\, w\, l^3 \left[\frac{\sin(\frac{1}{4}\pi - \frac{1}{2}\phi)\cos\phi}{\cos(\frac{3}{2}\phi - \frac{1}{4}\pi)}\right]^2.$$

With $\phi = 35°$ to correspond with the sand, it will be found that

$$\left[\frac{\sin(\frac{1}{4}\pi - \frac{1}{2}\phi)\cos\phi}{\cos(\frac{3}{2}\phi - \frac{1}{4}\pi)}\right]^2 = 0{\cdot}3125$$

and

$$\sin(\tfrac{1}{4}\pi - \tfrac{1}{2}\phi) \frac{\cos^2\phi}{\cos(\frac{3}{2}\phi - \frac{1}{4}\pi)} \doteq 0{\cdot}1455.$$

From Series V. it appeared that the latter coefficient was 0·147, and from Series VI. that the former was 0·291. The agreement therefore of this rough and semi-empirical rule with experiment is far better than might have been expected.

The reasons for the discrepancies from the theory and for the want of agreement of the different series of experiments amongst one another will now be considered.

It has always been assumed by previous writers that the tangential action across an ideal interface in a mass of loose earth is of the same nature as the statical friction between solids, and that when the tangential stress has attained in magnitude a certain fraction of the normal stress, the equilibrium is on the point of breaking down. That fraction is the coefficient of internal friction of sand, and is supposed to be equal to tan ϕ, where ϕ is the angle of a talus of the greatest possible slope. A little consideration will show that the hypothesis cannot be exact, even with an ideal sand with incompressible grains, and absolutely devoid of coherence. For imagine a mass of sand thrown loosely together; then if the grains are of irregular shape a certain portion of them will be resting on points and angles, thus occupying more space than they might do.

If the sand be now compressed, many of the grains will slip and rotate, and fall into interstices; in fact a considerable amount of re-arrangement will take place, and the density of the mass will rise considerably—by quite 10 per cent. if the re-arrangement be thorough, as found experimentally.

Even if all the grains were spherical a considerable amount of change would take place, and when they are angular of course much more. After a certain amount of pressure or shaking has been applied the grains could not be made to pack closer. This movement of the grains amongst themselves may be described as "settling."

Now if the maximum tangential stress across an interface, which is compatible with equilibrium, be compared before and after settling, it can be seen that they will be very different. For a grain of sand which is well embedded amongst its neighbours will require much more force to displace it than a grain resting partly on points and angles.

Hence it is clear that the coefficient of internal friction of sand is a function of the pressure, and not merely of the pressure then existing, but also of the pressure and shaking to which at some previous period that portion of the mass of sand has been subjected.

No mass of sand can be put together without some history, and that history will determine the nature of its limiting equilibrium. It is quite impossible to say how much these causes will vitiate any mathematical theory of the equilibrium of sand, but experience seems to show that the vitiation is extensive.

On considering these views, it seems reasonable to suppose that, when in Series I. the sand was built up in approximately parallel layers, it underwent a partial settlement, and on the average the grains of sand were not in positions which predisposed them to fall more one way than another. In Series II. the settlement was pretty completely accomplished by shaking and stirring. In both these cases it was difficult to state that there was any exact epoch of instability, and it is a highly plausible supposition that the first process, before a definite sand-slip, was a gradual "unsettlement" of the sand, in which one grain after another partially rotated, assumed a more open order, and caused the whole mass to occupy a larger volume. This gradual unsettlement would almost certainly take place along certain surfaces or narrow regions, which ultimately formed the seat of slipping. After the unsettlement had proceeded to a certain extent a visible slip would take place; probably this slip caused a partial resettlement, and then must follow repetition of the unsettling process and a fresh slip, and so on.

Now from these *à priori* considerations it seems as though the rough theory above developed, in which the equilibrium is considered of a wedge of sand, treated as a rigid body, should give results more conformable to facts than theories which treat the sand as a continuous medium. For there seems reason to suppose that the unsettling process, preparatory to slipping, only takes place over certain surfaces, and that in other regions the sand behaves like a rigid body. In all modes of building a mass of sand, something of this settling and unsettling must take place.

Consider now the experiments of Series III., where the sand was built up so as to form a talus sloping away from the door. The sand was lying in loose order, and each grain must have stopped whilst falling away from the door. On this occasion when the first sand-slip took place there was often a complete break down of the equilibrium.

The following may be offered as a plausible explanation of this fact: The grains having attained a more or less stable configuration for movements away from the door, had probably become jammed together so as to be arranged in arches with the convexity directed upwards and towards the door; displacements towards the door would therefore break up these arches, and equilibrium would not be again attained until the arch-like arrangement was re-established with the concavity towards the door. The sand being in loose order scarcely any preliminary unsettlement was necessary before the equilibrium broke down. Now in the experiments of Series IV. the sand was built up with the talus sloping towards the door. The sand was in loose order, and each grain must have stopped as it was rolling towards the door.

A fact which was noticed during the operation of building up the sand in a talus has not previously been adverted to, viz. that during the process the Author frequently saw or heard a very small partial slip, which occurred a few seconds after a dose of sand had been poured on to the talus, and when equilibrium had apparently been attained. He noticed in this series that, when one of these partial slips occurred near the end of the operation of filling the box, the tension on the cord had to be relaxed, below what might have been expected from the other experiments, before equilibrium broke down, and then the first slip was in general an excessively small one. The sand had probably settled into a configuration of more stability for displacements towards the door than if this partial slip had not taken place.

If the theory of the arch-like arrangement of the sand, suggested to explain the phenomena of Series III. be adopted, it seems that the reason of the anomalous results of Series IV. and of the smallness of the first slips can be explained. For according to this view the sand was built in arches adapted to resist motions towards the door, and when a grain or two was displaced from an arch, only a very small motion was requisite before the arch was re-formed. Moreover, if a partial slip had just occurred at the end of the operation of building up the sand, the arches were stronger because there had been more complete settlement, and thus a greater relaxation of tension of the cord was possible before there was displacement than if the partial slip had not occurred.

Whatever may be thought of these suggested explanations, the fact remains that Series III. gave large slips and fairly consistent results, and Series IV. small slips and inconsistent results.

It will not be necessary to advert here to the experiments of Series V. and VI., since they only differed from those which have just been discussed in the form of the surface of the sand.

In these experiments a material was purposely chosen as much like the ideal loose earth of the mathematician as possible: and although the experiments have been by no means so various and complete as is desirable, yet they have been sufficient to convince the Author, at least, that even with such materials the ideal laws of equilibrium are by no means obeyed. The coefficient of maximum internal friction is probably very different in various parts of a mass of sand, and is not in fact a constant at all; and the process of settlement and unsettlement is of such importance that it is not possible to regard the mass as incompressible; in many cases there is no definite phenomenon which can be called the break down of equilibrium; and, lastly, a rough empirical rule for finding the equilibrium of a wedge of earth in the rear of a revetment will probably give results more conformable to fact than the elaborate methods which treat the material as homogeneous. The Author wishes to ask engineers whether there are not in general some preliminary signs of the failing of a wall by cracks appearing first in the earth at the back; and whether, when a wall has begun to bulge out in places, that process does not frequently stop, and the wall remain sound for ever afterwards?

This is what would be expected from the above experiments, for the unsettlement of the earth appeared to be a preliminary in those cases which bore the most resemblance to actual earthworks, and the bulging of the wall may frequently give the earth an opportunity of settling into more stable equilibrium, and the pressure on the wall may thus be relieved. Now if these conclusions are sound with regard to an almost ideally perfect material like dry sand, with how much more force may they be applied to such materials as those with which engineers actually have to deal?

Imagine, for example, a revetment-wall, and that it is to be filled up at the rear with a substance like pitch. This is at first friable and powdery, and may lie in a talus, like sand. When the embankment is first made the pressure on the wall will be somewhat the same as though the substance were loose earth. But after a time the pitch will settle and bind, and the pressure on the wall will become the same as though the material were fluid. Now while there is certainly no kind of earth with such perfect viscosity as that of pitch, yet some clays approximate to it. In these cases the pressure on the wall will probably rise largely. But in the case of ordinary loose earth it is probable that the pressure is greatest at first, and that as the earth settles the pressure falls. If the earth is liable to become sodden with water and then dry again, the variations in pressure will probably be very large.

The Author believes that the incomplete experiments of which he has given an account are the first in which a record of the "historical element" has been carefully kept, and in this respect they seem to him to be worthy of record.

It is to be hoped that other experimenters may be induced to take up the subject, and to carry out experiments with other materials, and by other methods. But until this is done, and the conclusions stated above are shown to be faulty, the soundest view seems to be that engineers have no better practical course open to them than, neglecting the elaborate formulas which have been suggested, to work with semi-empirical rules such as those of Coulomb, and to allow a large coefficient of safety.

The Paper is accompanied by several diagrams from which the woodcuts in the text have been prepared.

(*Paper No. 1905.*)

"Villar Reservoir on the River Lozoya."[1]

By Edward John Theodore Manby, M. Inst. C.E.

The Isabel II. Canal, which conveys to Madrid the waters of River Lozoya, began originally at a point called Ponton de Oliva, where the required level was established by a dam 92 feet high. A supplementary dam, 16 feet high, was subsequently built at Navarejos, about 3 miles further up the river, and the canal prolonged up to that point; but the combined capacities of these reservoirs proved insufficient to meet the growing requirements of the capital, and it was decided in 1869 to construct a large storage reservoir at Villar, about 14 miles above Navarejos, capable of holding 20 million cubic metres (4,400 million gallons).

The construction of this dam began in June 1870 and ended in 1878. The dam is curved in plan to a radius of 440 feet, the convexity being of course on the upstream side. The total length of the dam at the top, measured along this curve, is 546 feet, out of which a length of 197 feet on the right bank is 8 feet 3 inches lower than the remainder, thus providing an outlet for the overflow of the reservoir. The overflowing water is directed down the valley, and kept from falling on the foot of the dam, by a guide-wall 60 feet long, running radially to the curve of the dam, from the inner end of the outlet. An iron bridge, consisting of twelve spans of 16 feet, runs across the outlet, carrying a roadway which continues along the top of the higher portion of the dam. It was necessary to provide a crossing for horses and foot-passengers over the dam, to replace the Villar bridge drowned by the reservoir.

The maximum height of the dam in the centre of the valley, from the bed of the river to the highest level the water can attain, *i.e.* the level of the overflow, is 162 feet 1 inch. The higher portion of the dam rises to 8 feet 3 inches above this level, and is provided with parapets.

---

[1] This Paper has been mainly compiled from information given in a series of articles in the "Revista de Obras publicas," Madrid, supplemented by additional particulars from other sources.

The figure of the cross-section of the dam can be very approximately described as follows:—The inside or upstream face is vertical from the line of the highest water-level to a point 84 feet 3 inches below, whilst the outer or downstream face batters at an irregular rate down to the same level.

The thicknesses down to this level run as follows:—

	Feet.	Inches.
At and above overflow level	14	9
„ 10 feet below overflow level	16	5
„ 20 „ „ „ „	19	4
„ 30 „ „ „ „	24	3
„ 40 „ „ „ „	30	2
„ 50 „ „ „ „	36	9
„ 60 „ „ „ „	44	8
„ 70 „ „ „ „	52	10
„ 80 „ „ „ „	61	4
„ 84 „ 3½ inches „ „	65	11

From 84 feet 3½ inches below overflow-level the inside face batters 1 in 3·57, and the outside face continues with a uniform batter of 1 in 1·163, till they respectively strike the rock in the bed of the river.

Two galleries run through the dam at a depth of 143 feet below overflow level. Each of these has a clear inlet area of 19 square feet divided into two compartments, closed by sluices governed from a central tower built on to the inner face up to the level of the roadway. These sluices are worked by hydraulic power derived from a spring found on one of the banks 2,000 feet distant from and about 200 feet higher than the dam. The strain on each sluice-rod with a full reservoir is 8½ tons.

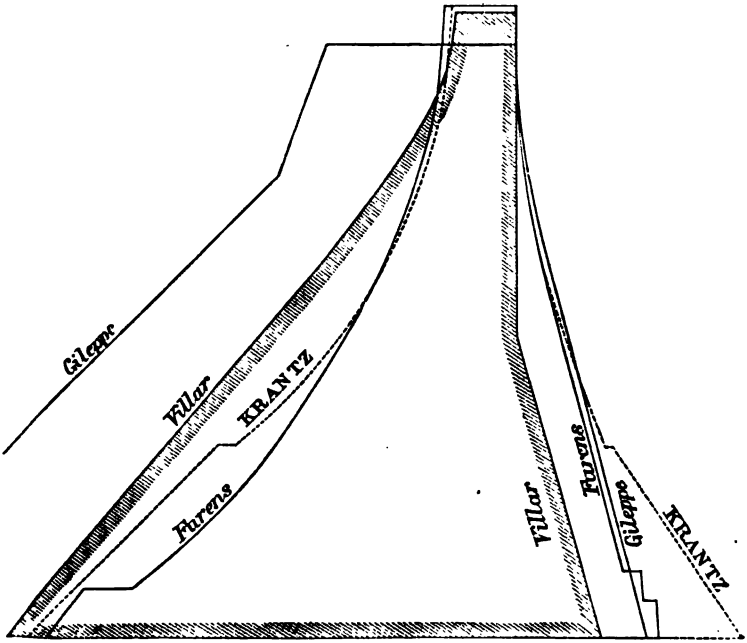
Besides these two galleries, four tunnels have been driven through the rocky sides of the valley to provide an outlet for flood-waters, and avoid, if possible, the higher overflow coming into use. Two of these issue from the same level as the galleries, a third one from a point 50 feet higher, and a fourth from a point 77 feet higher.

The whole of the dam is built of hydraulic rubble masonry, except the lower part of the tower and the top course of the dam, which are of ashlar.

The total cost of the reservoir, including the bridge and the tunnels, was £80,556. The materials were supplied by contractors, and cost £42,000. The construction was executed by day labour, and cost £38,556.

The sectional area of the dam from overflow-level down to a horizontal line 162 feet 1 inch below is 1,275 square yards, and may be considered as a fairly light one. The Furens dam, which is of about the same height (164 feet), only measures about 1,200 square yards in area; but the Gileppe dam, if carried up to the height of 164 feet (which contingency was contemplated and provided for by its designers), would present an area of 2,130 square yards. The type calculated in Krantz's work for a height of 164 feet has about 1,330 square yards area.

Fig. 1.



Comparative Sections of the Villar, Furens, and Gileppe Dams.

N.B.—The outer slope of the Gileppe section is continued to the base at the same angle.

The Villar dam is slightly narrower at the base than the Furens dam, and much narrower than the Gileppe dam, or Krantz's type. Its thickness at 162 feet below overflow is about 154 feet. The Furens dam measures 159 feet at the corresponding depth, according to the plate in Krantz's work, and the Gileppe 216 feet. The Krantz type, for a height of 164 feet, gives 185 feet as the proper thickness at base.

Fig. 1 represents the comparative outline of the sections of the Gileppe, Furens, and Villar dams. Krantz's type is figured in dotted lines. The outline of the Furens dam has been taken from Krantz's work. That of the Gileppe dam has been plotted from the dimensions given in a notice printed in these Minutes, vol. xlviii., p. 312. The Villar dam was designed and constructed by Mr. José Morer, Chief Engineer to the Spanish Government.

(*Paper No. 1906.*)

"Tracklaying in 1882 on the Main Line of the Canadian Pacific Railway."

By Henry Purdon Bell, M. Inst. C.E.

The contract upon which the operations to be described were conducted, extended from a point about 170 miles west of Winnipeg to a point 500 miles still further west, and was that class of work known as "prairie-grading." It must not, however, be supposed that the country through which the line passes is a uniform level. It became necessary in many places to use a large proportion of curvature and occasional gradients of 53 feet per mile, with quantities as high as 30,000 cubic yards of excavation to the mile. The embankments were 14 feet wide on the top.

The contract for these 500 miles was given by the Canadian Pacific Railway Company to Messrs. Langdon and Shephard, an American firm, and the organisation employed by them was the result of years of experience on similar works in the United States. The Railway Company bound themselves to forward all material and supplies to the contractors. This portion of the work was conducted by the General Manager of the Company, Mr. Van Horne. Taking into consideration the distance that the contractors were working from their base of supplies and material, the detention caused by excessive floods in the spring of 1882, the rapidity with which the distance from the base of supplies was extended, and many other disadvantages, great in the aggregate, it is but fair to say that the forwarding of material and supplies was a half of the season's work; and that the season's work, taken as a whole, has been considered by good judges the most successful of its kind yet executed on the American Continent.

TRACKLAYING.

When a train-load of sleepers was forwarded, it was unloaded by thirty men, who re-loaded the same on thirty-five two-horse wagons, which were sent to the front as soon as loaded.

At the end of the track six men unloaded these sleepers, and twenty more put them in place ready to receive the rails as they were removed from the iron car.

The rails were unloaded from the trains in quantity equal to ½ mile of track at a time, and sleepers for the same distance. Twelve men unloaded the rails, and when the train moved back these same men reloaded these rails upon the iron cars, which were sent forward, with two horses to each car. These horses travelled as fast as possible to the end of the track.

The iron car resembled in its proportions an ordinary low four-wheeled truck, and was furnished with four rollers to neutralise the friction encountered in pulling the rails from the iron car and dropping each length in place upon the sleepers. Four of these cars were used in getting the rails to the end of the track, and the method of operating them was as follows:—At the beginning of a day's work the four iron cars stand upon the track in front of the boarding-car, the track being clear to the end. The foremost car is taken to the pile of rails, there loaded, and sent to the front at once. Another iron car is then pulled forward to take its place. By the time that the car first forwarded has been unloaded, the second car loaded is upon the track immediately behind it. The first car is then lifted up, with its side resting on the ends of the sleepers clear of the rails, and the loaded car is passed by it to be handled by the track-layers. The first car, on its return to the rail-pile, meets there a third car loaded, and is again lifted as before described, and the loaded car drawn forward, leaving the third car ready to start for the end of the track. Briefly described, the operations of these cars consist of passing and re-passing each other, in the mode described, between the pile of rails and the end of the track. Immediately behind the forward iron car at the end of the track are twelve men putting on fish-plates and full-bolting them; these men do nothing else, but they are obliged to keep pace with the laying of the rails, in order to leave the track-joints in position for the car coming behind.

Behind the bolters are six men distributing track-spikes, commonly called "spike-peddlers."

The mode of distributing sleepers in America originated the use of the term "line-side of the track"; that is to say, that side of the track upon which the line is stretched with which the sleeper-ends are made to coincide, and is generally the right-hand side facing the track-end, but which side is immaterial. The side opposite the line-side is called the "gauge-side." Track-spiking is divided into joint, quarter, and centre-spiking.

After the six men described as spike-peddlers come two men called line-spikers, accompanied by a nipper, who holds up the end of the sleeper with a bar while the spikes are being driven. These two men spike the sleeper upon either side of the rail-joint, but only upon the line-side of the track. Opposite these two spikers and the nipper is a similar gang to spike the joints upon the gauge-side of the track, first using the gauge to place the rail-ends the proper distance apart. Immediately behind the two line-spikers come the two quarter-spikers and a nipper, and opposite to these another set of gauge-spikers. Behind these again are the centre-spikers and nipper, with their gauge-spikers opposite, the duty of each gang being exactly the same.

It will therefore be seen that the gauge of the track has been tested at five different points within the length of each pair of 30-feet rails, and is practically perfect.

To each 30-feet rail are fifteen sleepers 2 feet apart from centre to centre, and upon each side of the track are sixteen spikers and eight nippers, forty-eight men in all, including all the line- and gauge-spikers before-mentioned. The spiking is therefore done in exact rotation, by one gang following the next ahead to a sleeper in similar position each successive length of rails. Behind the gangs just described are six men and a foreman, throwing the track into exact line, which completes the operation of tracklaying. To give an idea of the speed and precision attained by this method of tracklaying, it may be stated that during the month of August 1882, $92\frac{6}{10}$ miles were completed, the maximum distance in any one day being $4\frac{1}{2}$ miles. Mr. Donald Grant, who was in charge of operations at the end of the track, offered to lay, for the Members of the Dominion Press Association, who visited the work in August 1882, 1 mile of track in one hour. The Association remained upon the ground for half an hour, and saw the completion of $\frac{1}{2}$ mile. After the track has been properly lined, the work next in order is called surfacing, which consists of levelling lumps, filling holes, and bringing the rails parallel to grade, or as near as possible thereto. This is effected by raising any low portions of track and re-bedding the sleepers with material taken from the slope shoulders on each side. The form of embankment thus made is called a rain-section, and this last operation leaves the track in order for the running of trains.

Briefly summarising, the tracklaying gang consisted of—

30 men unloading and reloading sleepers.
6 „ „ sleepers from teams.
20 „ placing sleepers to receive rails.
12 „ pulling rails from iron cars to sleepers.
6 „ spike-peddlers.
12 „ bolters.
6 „ runners with iron cars.
12 „ unloading and reloading rails.
32 „ spikers.
16 „ nippers.
7 „ liners.

159 in all.

Besides these, there are thirty-five teams and thirty-five teamsters distributing sleepers under the direction of two foremen; making a grand total of one hundred and ninety-six men and seventy horses, in and connected with, the business of tracklaying.

OBITUARY NOTICES.

MR. CHARLES EDWARDS AMOS was one of the last links connecting the old-fashioned millwright with the modern engineer. He was born at March, in Cambridgeshire, on the 27th of November, 1805, his father being Mr. Jonas Amos, who had married the daughter of a millwright and carpenter of the district. Shortly after his birth his family removed to the Wildmore Fen, in Lincolnshire, where the elder Amos became manager on the farm of a Mr. Clements, of Horncastle. This occupation was not very different from that of a colonist at the present time; for in those days, owing to the isolation of the district and the general absence of drainage and cultivation, fenland might be purchased in fee-simple for what would be but a year's rental now.

Such education as the place afforded young Amos managed to acquire, albeit the bad state of the roads, and the passage of the river Witham rendered his attendance at school in winter very desultory, while in summer the value of children's labour stopped his lessons entirely. He was, however, so quick and intelligent that, in spite of these drawbacks, he soon distanced his comrades in the acquisition of the three R's, and was allowed to amuse himself with an old folio "Life of Josephus" during the time the master instructed the rest of the class.

On the removal of his parents to Upwell, where his father became farm manager to Mr. William Bacon, young Amos went to regular farm work, and became, while literally "whistling at the plough," thoroughly conversant with agricultural implements and operations, a knowledge which years afterwards stood him in good stead. At the age of about eighteen years, he apprenticed himself to Mr. John Wilkinson, millwright and machine maker of Elm. Starting on a weekly wage of 10*s.* 6*d.*, with which he had to maintain himself, he soon became so far useful to his master as "leading-hand" on out-door work as to be placed frequently in charge of such jobs, and remained about four years, during which he acquired a competent knowledge of windmills, sluice-work, threshing-machines, and other mechanism incidental to a country business. Being now twenty-two years of age and an experienced journeyman, he, accompanied by a fellow-apprentice, determined to go "on tramp," and soon found employment at the workshop of

Mr. Beamont of Ramsey, Hunts, whose practice was almost exclusively confined to fen-work. At that time the fens were drained mainly by windmills actuating scoop-wheels, following the practice imported by the old Dutch settlers. The introduction of steam has during later years dispensed with many of these windmills, but at that period steam-power, machine-tools, and the host of other appliances, were unavailable for the facilitating the work of the millwright, and he was compelled to rely upon his own resources from the preparation of the timber, iron, and other rough material to the erection of his finished work.

The repair and construction of windmills, and scoop-wheels sluices, &c., over a large extent of country, became at length monotonous; and Amos, notwithstanding kind offers on his master's part, decided to see some different class of work, and accordingly went into the shop of Mr. John Clark, millwright of Houghton, Huntingdonshire, where, in the practical construction of corn-mills, water-wheels, windmills, and tannery and brewery work, abundant field for experience was found. The old millwright system, where the master, his sons, and his journeymen worked side by side in the same shop, rendered the journeyman's post an honourable one; it secured good work, and gave a facility in the use of carpenters', fitters', and smiths' tools, which even in these days renders the old millwright, where he can be found, a valuable man in any factory.

About the year 1829 Amos entered the employment of Mr. Joseph Jordan, millwright of Hertingfordbury, Herts, whose practice was of a similar character to Mr. Clark's. Here he was employed in the erection of several water corn-mills then in progress. Mr. Jordan was executing some steam-engine work and other machinery for Mr. Thomas Creswick, of Hatfield Paper Mills, and Amos was sent to erect and start it. Mr. Creswick was one of the most active and enterprising papermakers of his day, and his name and productions stood deservedly high, and wishing to introduce several new improvements and processes into his manufactory and machinery, and keeping them private for trade purposes, he was anxious to have a steady and skilful constant hand exclusively to himself. Amos, although nominally in Mr. Jordan's employment and receiving his pay, had virtually so acted for a considerable time, not going home for weeks together. At length Mr. Creswick offered him this situation, which, after consultation with Mr. Jordan, was accepted, in preference to embarking in business and taking over the goodwill and trade of Mr. Cotterill, of St. Albans (who died about that time), which he was strongly tempted to do by several of the leading millers, brewers, &c., of that district.

In the autumn of 1835, Mr. Creswick purchased the Iron Mills, Wandsworth, Surrey, which had been a rolling-mill and foundry where, among other things, the iron columns used by Mr. Nash in the formation of the Quadrant, Regent Street, were cast. In view of the contingency of Mr. Amos embarking in business for himself, on the completion of the Wandsworth Paper-Mills, he received instructions from Mr. Creswick to place some of the work in the hands of Mr. Henry Pullen, millwright and engineer of that town, at prices sufficiently liberal to induce him hereafter to devote his energies and the resources of his establishment to the requirements of the paper-mill, when its owner could no longer command the aid of his own engineer. In consequence, designs for two water-wheels were handed to Mr. Pullen for execution, but they had barely been commenced before the sudden death of that gentleman frustrated the arrangement. In this emergency it was suggested that Mr. Amos could not do better than establish himself as a millwright and engineer at Wandsworth, in which case his aid would be available for the paper-mills, and its machinery generally could be constructed by him.

It was about that time that Mr. George Dives contemplated the erection of a new steam-engine and other gear at his corn-mills, Battersea, and made a contract with Mr. Pullen for their installation; his death at an early stage of the business, however, brought matters to a standstill, and under these circumstances Mr. Dives requested Mr. Amos to complete the affair. Although the advantages of expansion in the working of steam-engines were known, not much practical use had been made of the knowledge. With the exception of the Cornish pumping-engines, most engines, ashore and afloat also, were worked with steam at a few pounds only above atmospheric pressure, which was expanded slightly in the cylinder and then condensed, engineers generally bestowing attention more on the details of construction than on the economical effect obtainable. The general principle of the "Woolf," or compound engine, had been set forth about 1804, and its progenitor, "Hornblower's" engine, dated as far back as 1781, but very little practical use had been made of it, and its employment in presence of the then existing practice was attended with some hardihood. Nevertheless a compound beam-engine was erected at Mr. Dives', the details of which were worked out by Mr. Amos, and the economical effect of corn ground for coal consumed was, at the time, remarkable. The engine in question is (or was up to a short while ago) still at work, and its performance bore respectable comparison with the duty of engines of the present time. Several of the constructive details, as continuous bedplate, valve-motions, mode of framing, &c., were in advance of the day, and have been generally employed since.

The success of Mr. Dives' engine attracted a good deal of attention, and among other persons, who through it came in contact with its constructor, was the late Mr. James Easton; the acquaintance resulted in a partnership, which commenced in 1836.

The compound engine, on the system tried at Mr. Dives' mill, became a speciality in the hands of Easton and Amos; but, although many such engines were built, the principle did not make general headway, and Messrs. I. and E. Hall, of Dartford, Messrs. John Wentworth and Sons, of Wandsworth, and they remained the principal constructors, so far as the south of England was concerned. The little 4-HP. engine at the Polytechnic Institution, built about this time by Mr. Edward Humphrys, M. Inst. C.E. (afterwards of Messrs. Humphrys and Tennant), was about the best known representative of this class; and it was sold by auction on the 1st of March, 1882, on the dismantling of the Polytechnic Institution, apparently in as good order as when it started.

While advocating strongly the merits of compound engines he was not insensible to the claims of single cylinder expansion, and he always stated that he believed he was the first man who successfully applied a cut-off slider working direct on the back of an ordinary slide for expansive working. It is to be regretted that the precise date when this was brought out is unknown. At the Reform Club-house such a device was used, a cut-off slide worked on the back of an ordinary slide-valve, and closed ports formed therein. Variations of the grade of expansion being obtained not only by swivelling the expansion eccentric around the shaft, so as to alter the time of cut-off, but by simultaneously varying the travel. It is figured and described in the 'Civil Engineers' and Architects' Journal,' of February 24th, 1844, and Mr. George Spencer, who describes it, gives the date of the application at the Reform Club as probably earlier than 1840. However this may be, this is doubtless one of the earliest instances of "double slide expansion," and is probably as old, if not older, than "Meyer's System."

Soon after the establishment of the firm, the remodelling of Mr. Walker's oil-mills at Dover was placed in their hands. For driving some of the machinery a pair of side-lever marine engines from one of the old packets, the "Royal George," was employed. Mr.

Amos removed the cylinder of one, and compounded it with the other engine by putting a smaller cylinder in its place. As the cranks were at right angles the disused cylinder was placed between the engines as an equaliser.

His old experience of paper-making was also turned to account, and several of the principal mills of the country were either built, remodelled, or added to under his auspices. In 1849, in conjunction with Mr. Moses Clark, of Mr. Wm. Joynson's mills at St. Mary Cray, Kent, he took out patents for a new knotter or pulp-strainer, also for a single-sheet cutting machine, which rapidly found favour both in the English and continental paper-mills; and a further patent for an automatic regulating valve, for giving steam of constant tension, notwithstanding variations of boiler pressure, found equal favour among steam-users in the West Indies and elsewhere. Other inventions relating to the paper manufacture, notably the revolving rag-boiler for boiling direct with high pressure steam in a closed vessel, also a method of sizing and drying, and sundry incidental details, were successfully brought out.

Hydraulics, and the practice incidental thereto, were favourite matters both with Mr. Amos and his partner. The supplying of towns and water-works machinery became special objects of attention, and a large practice resulted. One of the consequences was the revival of the use of the "bucket and plunger" or "double-acting pump." This had become so far obsolete that an amusing controversy occurs in the Civil Engineer's and Architects' Journal for 1849, wherein one of the disputants claimed its application at the then Richmond Water Works as a new thing. The point was settled, however, by a letter from the firm in that journal, under date June 14th, 1849, showing that the device had been revived and applied by them in the machinery erected at the Government Water Works, Trafalgar Square, in 1844.

The consideration, of a difficult case, in which the flow of water through a very long main was involved, led Mr. Amos to work out certain rules as to the influence of varying land-contour and main-contour upon such flow. The leading principles thus enunciated are given in "Practical Hydraulics," by Thomas Box, with a suitable acknowledgment by the author of the source from whence he derived them.

On the retirement of Mr. Josiah Parkes, M. Inst. C.E., the firm were appointed Consulting Engineers to the Royal Agricultural Society. In this capacity, Mr. Amos found congenial employment. At the Norwich Show, 1849, he established the system of engine-trials on the "Prony" brake, which has largely contributed to the high duty and general excellence of the modern portable engine. The invention of a dynamometer, whereby the dynamic effort involved in working any winch-driven implement was recorded by automatic lever and spring-balance movements, was the means of securing special recognition and a special gold medal from the Society in 1849, a circumstance which has escaped record in the various descriptions published of the Society's testing apparatus. An apparatus for ascertaining the power consumed by horse-gear threshing-machines was brought into use at the Exeter Show in 1850, and a rotary dynamometer (whereby the power consumed by any machine driven by steam or other prime motor was recorded) was invented and brought into use at the Lincoln Show in 1854. The various modifications in the form of dynamometer used by the society were mostly due to Mr. Amos, who ultimately, in 1864, produced one for testing the draught of steam-ploughs, cultivators, and the like, by recording the stress occurring continuously on the steel wire hauling-rope. The integrating gear required much care in manipulation, but with such care the apparatus acted well. A complete illustrated description of it, by the inventor, is given in the 'Journal of the Royal Agricultural Society of England,'[1] and the general principles embodied in it are fully explained. Some minor apparatus for testing the stress on plough anchors and various other purposes were likewise arranged by him for the use of the Society.

Mr. Amos relinquished the post of Consulting Engineer after the Oxford Show in 1870, and was on retirement presented with an illuminated address and a gold medal, in recognition of his services to the Society.

In the arrangement for the cable-laying machinery for the old Atlantic cable, in 1857, employed upon H.M.S. "Agamemnon," and the U.S. frigate "Niagara," a committee was appointed to assist the engineer, Mr. (now Sir Charles) Bright, M. Inst. C.E., comprising the late Mr. John Penn, the late Mr. Joshua Field, the late Mr. Lloyd of the Admiralty, and Chief Engineer W. E. Everett, of the United States Navy, upon the latter of whom a good deal of the oversight of the construction devolved. In consultation, Mr. Amos, in whose firm's hands the construction had been placed, suggested the placing of the paying-out drums in duplicate, so as to form a self-fleeting windlass, a device he had employed some years before with success at the Rhyl swivel

---

[1] Second Series, vol. i., p. 204: "A Description of the Newcastle Dynamometer."

bridges, on the Chester and Holyhead Railway. It answered the purpose, and although in after years an ordinary single drum has been commonly used, on account of greater simplicity, with an inclined plough-piece to thrust over the coil, there is doubtless more strain and punishment of a cable in this case than with the more complex arrangement of the self-fleeting or double-drum. For picking-up purposes, however, the double drum is still frequently employed.

The merit of the "Dynamometer" arrangement, which was brought out and designed for this expedition, is entirely due to Mr. Amos. The principle of the invention, in brief, lies in deflecting, by a wheel loaded with known weights, the cable as it passes away to the taffrail, the load coming equidistant between two fixed pulleys, and the angle formed by the cable (or, in other words, the amount of deflection) will, in accordance with the well-known laws of parallelogram of forces, indicate the stress upon the cable at any time, which may be read off upon a scale. The experiment was tried on a temporarily-fitted apparatus at Gravel Lane, Southwark (where the machinery was erected), and the details were very completely worked out for the two cable-vessels.

The invention of Appold's centrifugal pump, and its public exhibition, in 1851, in Hyde Park, was the means of establishing a lasting friendship between the late Mr. J. G. Appold, Assoc. Inst. C.E., and Mr. Amos. The centrifugal pump formed a favourite subject of speculation with the latter, who bestowed much time and thought on the investigation of the general laws governing the action of such pumps, and in experimenting upon them under varying conditions.

In the erection of the ship-elevator, invented by Mr. Edwin Clark, M. Inst. C.E., at the Thames Graving-Dock, Victoria Dock, wherein the hydraulic arrangements were placed in the hands of his firm, Mr. Amos introduced a three-cylinder compound-engine and a system of working the hydraulic pumps in groups, throwing off in succession as the lift proceeded and the stress increased; there were also some specialities in the arrangement of the valve-gear in the valve-house for operating the hydraulic cylinders.

In conjunction with Mr. John Francis, Mr. Amos patented a machine for dressing slates, which was successfully tried at Penrhyn Quarries, and came largely into use in slate quarries generally afterwards. A number of other inventions might be described, but enough has been stated to show the versatility of his mechanical ideas. One instance will suffice to show that little matters did not escape attention. In lead-pipe machinery, the practice had been to place the lead-pressing cylinder above the hydraulic cylinder, and so draw out the pipe upwards; much care had to be taken in adjusting the "triplet" truly concentric with the "die," so that the pipe should be of equal thickness, and frequent breakages were the result of any lack of attention in this respect. In conjunction with Mr. Hanson of Huddersfield, Mr. Amos reversed the order of things, forming the column of the press hollow, the pipe being drawn downwards: the "triplet" was further hung pendulous and left free instead of being rigidly confined; the result was that as the viscid, partially fluid metal, had no particular reason for flowing down one side of the triplet more than another, the triplet itself hung naturally concentric with the die, and an equal thickness of pipe ensued without undue strain or risk of breakage. The press, which was peculiar in this respect, was worked for many years in this way, and apart from its regular business, was frequently used for experimental purposes; for instance, in pressing trial composition for "Hale's rockets," about the date of the Crimean war, by the Woolwich authorities; and in pressing lead rod on trial, preparatory to the invention of the Minié rifle-bullet machinery, by Mr. (now Sir John) Anderson, M. Inst. C.E., the machinery superintendent at Woolwich Arsenal.

In 1866 Mr. Amos retired from business, but he did not by any means become an idle man. The chairmanship of the Sutton Gasworks, a directorship of the Grays Chalk Quarries Company, and other industrial pursuits occupied a large part of his time. He also found leisure, at the request of the Society of Arts, to devise and construct a dynamometer for testing the tractive force required on various pavements in London with a given load. He was a good witness in cases of disputed patent-right, and in other matters involving vexed questions in mechanics.

A severe attack of illness in the winter of 1881 effectually undermined his naturally strong constitution. Recovery was very slow, and though in the following spring it was hoped that he had overcome the enemy, his apparent restoration proved to be but the flickering of an expiring flame. On the 12th of August 1882 his spirit passed away without pain or pang of any sort, when in the seventy-seventh year of his age.

Mr. Amos was elected a Member of the Institution of Civil Engineers on the 22nd of May 1855, and was also connected with most of the associations devoted to the furtherance of practical science. He was a juror at the Paris Universal Exhibition of 1855 and International Exhibition, London, 1862, and, among several other distinctions, received a gold medal and diploma from the Agricultural Exhibition of Sweden and Norway in 1871, and was shortly after presented by Carl XV. with the Cross of the Order of Vasa.

Mr. Amos was a typical Englishman of a high class. In him the intuitive sense of mechanical principles, perhaps inherited, but in any case amounting almost to genius, with clear-headedness, enterprise, energy and determination more than compensated for deficiencies of early education, and combined to land him at a goal in advance of that reached by many men who start with far greater advantages. When to these qualities are added sterling uprightness and moral worth, the value of such a man passes the limited bounds of his individual sphere, and he becomes a pillar of the commonwealth and an honour to his country.

MR. ROBERT BRIGGS was born in Boston, U.S.A., on the 18th of June, 1822, and was educated in the public schools of that city, attaining special proficiency in mathematics. At the age of seventeen he entered the office of Captain Alex. Parris, a civil engineer and architect of Boston and Charlestown, N.Y., and presumably for three or four years was under training as a pupil, although he most likely received a salary, the English system of premium-pupilage not obtaining in the United States. In any case, this first experience of engineering work must be considered as instructional, for it preceded a practical education which his death alone terminated, and which ranged over all, and more than all, branches that in this country are considered as constituting the profession of an engineer, from the conduct of hydrographic surveys to the making of sugar-mill machinery; from the draughtsman's board to the editor's table. This varied career, during which the longest engagement in one department seems to have been nine years, the others being much shorter, began in 1847, under Mr. Charles Hastings, C.E., who secured Mr. Briggs' services for a few months to assist him in laying out railways in Massachusetts; but Briggs soon left surveying to become "Constructing Engineer" to the Glendon Rolling-mill, a notable establishment then being built at East Boston. On the completion of that undertaking he opened an office in Boston, which he retained for two or three years while in practice for himself. In 1850 he went to Newton, New Jersey, as superintendent engineer of the works of Messrs. Bird and Wild, but a year later reverted to private practice, and so remained until 1854, his occupation being, in his own words, of a "desultory" character. In 1854–55 he was manager of the well-known Renssellaer Mill at Troy, N.Y. But again one year of settled routine seems to have been enough for his active temperament, and he found more congenial work as assistant to General Meiggs in the Washington Aqueduct and the National Capitol. Next he became one of the firm of Nawn, Dodge, and Briggs, makers of heating-apparatus, a branch of engineering in which he always took much interest, and which, by his scientific acquaintance with the rules governing the flow of air, he greatly assisted to raise from the rule-of-thumb of the plumber and glazier to its present recognised well-secured domain. It is probable that the seductions exercised by his mathematical acquirements interfered with commercial success, for the partnership ended after a year's duration, and Mr. Briggs became superintendent of the Pascal Ironworks of Messrs. Morris and Tasker. This was the most successful and congenial appointment of his professional life, and lasted from 1860 to 1869, when he accepted a similar post at the Southwark Foundry in Philadelphia, an offshoot of, if not actually affiliated to, the Pascal Works. During these years Mr. Briggs made special study of the theory of the centrifugal fan; and thus originated his connection with the Institution of Civil Engineers.

In November 1869, he presented to the Society a Paper "On the Conditions and the Limits which govern the Proportions of Rotary Fans,"[1] which was read to the meeting on the 17th of the following May. This essay was a philosophical inquiry into the action of these machines, of great interest and value, not only in regard to the ventilating-fan, but also of its analogue the turbine. The Paper was awarded a Watt medal and a Telford premium. On the 4th of February, 1879, he was elected a Member of the Institution, and about this time joined the Institution of Mechanical Engineers.

Mr. Briggs' wide experience, eminently fitted him for the position of engineer-editor of the 'Journal of the Franklin Institute,' which he filled for several years from 1875. He subsequently established himself in Philadelphia as a consulting engineer, devoting himself specially to the design of heavy machinery and ironwork; the application of heat in the arts, and the design of works for gas and water supply. But he achieved only moderate success, as that sort of private practice is not much in vogue in America.

---

[1] Minutes of Proceedings Inst. C.E., vol. xxx., p. 236.

In 1880 he became consulting assistant to the United States Chief of Engineers, his duties being mainly connected with the maintenance of the rivers and harbours, a department of engineering chiefly under government control. When no longer officially connected with the Franklin Institute, he continued to enrich its 'Journal' with contributions from his facile pen, and his other literary work was plentiful. One of his Papers, read before the American Society of Civil Engineers, on the "Ventilation of Halls of Audience,"[1] attracted considerable attention. In it he urged that American engineers should discard European practice in this branch as unsuited to the conditions both of climate and physical constitution of the population; and he referred to the well-authenticated fact that the modern American requires a temperature of not less than 70° Fahrenheit for comfort, albeit his British congeners are satisfied with one of 10° less. In 1881 Mr. Briggs presented to the Institution of Civil Engineers an elaborate communication on American practice in steam-heating.[2] This was read, as a posthumous Paper on the 21st of November, 1882.

Of Robert Briggs' attainments as an engineer the foregoing brief record will bear some witness, but it will be enhanced by the following tribute to his great abilities, extracted from a letter of Mr. Henry R. Towne to the 'Iron Age': ". . . One of his most notable traits was the comprehensive scope of his knowledge, which covered almost the entire field of engineering, both civil and mechanical, and included much also of metallurgy, chemistry, architecture, and applied sciences. On almost any topic under these many heads he could discourse as a master with a minuteness and familiarity astonishing to any but those who knew what an extraordinary range was covered by his own personal experience in connection with mechanical and industrial operations, and who knew also how far these were supplemented by professional study and reading, continued uninterruptedly during the forty years of his business life. Added to these were the advantages of a good early education, an exceptional aptitude for mathematics (in which he excelled), and a very retentive memory. . . . His temperament was placid and equable, and his companionship a pleasure which his intimate friends at least can never forget." Among the latter, those of English nationality will certainly allow that the above meed of praise is in nowise exaggerated.

Mr. Briggs had never enjoyed very robust health, and of late

---

[1] Minutes of Proceedings Inst. C.E., vol. lxv., p. 470.

[2] *Ibid., ante*, p. 95.

years it was apparent that his vital forces were gradually failing. He died on the 24th of July, 1882, at his mother's residence at Dedham, Pennsylvania, after a lingering and painful illness, having but recently completed his sixtieth year.

MR. CHARLES LENNOX DAVIES was the son of Captain Richard Plummer Davies, R.N., descended from an old family who were large proprietors in County Cork since the reign of Queen Elizabeth. He was born at Mallow in July 1822, and was educated by the Rev. Julius Armstrong, of that place. In 1837 he was articled to the late Mr. Charles B. Vignoles, Past-President Inst. C.E., and was appointed to assist Mr. John Collister, Mr. Vignoles's Resident Engineer, on the North Union Railway. On the completion of his pupilage in 1842, he remained with Mr. Vignoles as assistant till 1847, principally employed on the Sheffield and Manchester Railway. After a short period served on the Waterford and Wexford line, he in 1850 emigrated to Canada, which at that time offered a promising field for young engineers, on account of the impending great development of the railway system of the colony. Mr. Davies was at first occupied in a government survey, but speedily found himself restored to his familiar railway work, as Assistant Engineer in charge of a section of the Grand Trunk Railway, west of Toronto, during its survey and subsequent construction. He was thus occupied for three years, and afterwards passed a second three years in similar work for the Great Western Railway of Canada and the Canadian Government. He continued his government service for a further term of two and a half years on exploration surveys of the little-known regions east and north of Lake Superior, being in charge of the staff deputed to survey a new line of road through the Hudson's Bay Company's territory. In the intervals of his official engagements in Canada, he practised locally as a civil engineer, making private surveys, laying out village lots, conducting arbitration cases, and locating streets and roads in new towns.

In 1862 Mr. Davies returned to England, and thence, until the spring of 1867, acted as Resident Engineer for Mr. H. Conybeare, M. Inst. C.E., on the Aberystwith and Welsh Coast Railway.

In January 1868 he entered the Indian Public Works Department, as an Executive Engineer, third grade, and a few months afterwards was posted to the Nuddea Division, South-Eastern Circle, under Mr. H. Leonard, M. Inst. C.E. A year later he was transferred to the Lower Ganges Circle, in which he remained until November 1870, when he was appointed to the Irrigation Branch, Cossye Division, S.-W. Circle. He afterwards, until 1876, had various charges, chiefly in the Irrigation and Canal Divisions. One of his most important and interesting works was the survey of the Kootoobdeea island in 1874. It was apprehended that this island was slowly being submerged, and the resolution was formed to make an embankment 40 miles long to arrest the coast action. In this work Mr. Davies made the survey and estimate, the latter amounting to 170,926 rupees. In 1872 he had been promoted to Executive Engineer, second grade, and retained that rank until he resigned the service in 1878, having previously obtained leave of absence for two years on account of ill-health, From that time until his death, which occurred on the 16th of August, 1882, Mr. Davies remained at the home of his family. The Garland, Mallow, County Cork. He was elected a Member of the Institution on the 24th of May, 1864.

Though not destined to attain to eminence in his profession, Mr. Davies was a man of sound ability, and his varied experience of all classes of engineering work made him a valuable executive officer. His character for sterling integrity and honour was high, and, united to an unblemished professional reputation, were qualities of heart and mind which made his loss felt by others than those private associates who knew most of an amiable and genial man.

MR. HENRY HOOPER was born in the year 1828. He began his education at the Streatham Academy, and continued it—with special reference to his after-career—at King's College and the Engineering School attached, also passing a year in France during the interval before entering the Applied Sciences Department. On the 29th of October, 1849, he was articled for two years to the late Mr. James Simpson, Past-President Inst. C.E., and afterwards remained with Mr. Simpson as an assistant on various water-works under construction until June 1856. He was next employed for three and a half years, until the end of 1859, as engineer to Messrs. Rennie, Logan, Thompson and Co., in charge of the Newport Dock Extension works. This engagement led to Mr. Hooper being introduced to Mr. James Brunlees, President Inst. C.E., by whom he was appointed assistant in connection with the construction of the iron piers at Southport, New Ferry, and New Brighton. These structures, which were of an important character, being also used as landing-stages, were described by Mr. Hooper in Papers read before the Institution in 1861 and 1869.[1] During his engagement with Mr. Brunlees, Mr. Hooper was also employed on the Lynn and Sutton-Bridge Railway, Rhyl Pier, Kelso Drainage, and other works, besides many surveys for works of a similar character elsewhere. Early in 1868 he entered the Public Works Department of the Indian Government as an executive engineer of the third grade, and was subsequently promoted to the second and first grades. During his service in India Mr. Hooper was employed in the central provinces, principally in the Irrigation Department, and although his name is not connected with any large works of construction, yet he earned a high character for his care and diligence in the preparation of projects and estimates for carrying out extensive works of irrigation, for which he received, on more than one occasion, the commendation of the supreme Government. He was also engaged in the railways of the Central Provinces, and in the buildings and roads branch, for a short period previous to his resignation in 1876.

After his return to Europe he ceased, on account of ill-health, to take any active part in his profession. He died on the 27th of April, 1882.

MR. JOHN MURRAY,[2] whose connection with the Institution extended over half a century, was born at Kelso, on the 12th of December, 1804. His father and grandfather were civil engineers of some repute in their day, and it was early determined that he should follow in their footsteps. He was accordingly sent to London, where he became an articled pupil in the office of the elder Mylne, who had a large practice as a dock-engineer. The beginning of Mr. Murray's independent career dates from 1831, when he left London to take up his appointment as engineer to the River Wear Commissioners at Sunderland. This office was one just suited to his special abilities, and during its tenure he laid the foundation of a reputation as a hydraulic engineer very little, if at all, inferior to those of the Walkers, the Rennies, and the Hartleys of the last generation.

---

[1] Minutes of Proceedings, Inst. C.E., vol. xx., p. 299; vol. xxvii., p. 217.

[2] For much of the information in this memoir the Institution is indebted to Mr. H. H. Wake, M. Inst. C.E., the present engineer to the River Wear Commissioners.—SEC. INST. C.E.

At the time Mr. Murray entered upon his duties, the state of affairs in regard to Sunderland harbour was far from encouraging. Its condition had exercised the minds of the inhabitants for generations, and with commendable perseverance they had endeavoured, often under trying circumstances, to effect improvement. From time to time the most eminent engineers of their day had been called in to give their advice, and divers were the schemes which had been proposed, and in some instances proceeded with. So far back as July, 1748, Labelye recommended the contraction of the channel at various points, and the construction of stone wharves to increase the scouring power of the stream, the reduction of the then several channels to one by wharfing, and the construction of a north pier to prevent the formation of sand banks at the harbour entrance. In 1780, Smeaton approved a plan prepared by Mr. Shout, engineer to the Commissioners of that day, for the rebuilding and extension of the old pier. Fourteen years afterwards the advice of Dodds, another engineer, was sought. Jessop, reporting in 1807, advised an extension of the piers and a contraction of the entrance, to make the harbour quieter within, and to increase the depth of water over the bar. Prior to this, however, the construction of a north pier had (in 1788) been commenced by Shout, and completed in 1802 by his successor, Pickernell, and a lighthouse had also been erected on the end. In 1819 an Act was obtained for an Admiralty survey of the river as far as Biddick Ford. This was undertaken by Mr. Giles, under the direction of Mr. Rennie, and was afterwards completed by Sir John Rennie. By this survey reliable data of a most valuable kind were obtained. Sir John Rennie also prepared plans for the construction of a new south pier, and the works were commenced by the Commissioners' engineer, Mr. Milton, Mr. Murray's immediate predecessor. They were in progress when the latter came to Sunderland, and were completed under his direction. In connection with these works, Mr. Murray designed and carried out the formation of the Polka Basin, as a place in which the waves might spend their force before getting up the harbour. The value of this basin is demonstrated in every storm that assails the north-east coast, for when a vessel has once got safely over the bar, a few minutes suffice to bring her into smooth water. Describing the condition of the harbour when he became engineer, Mr. Murray, speaking many years afterwards, said he "found the lower reach, between the business part and the pierhead, in a very tortuous condition, with a shallow draught of water through—in places deep holes, but generally shallow. Vessels used frequently to take the ground, sometimes in great number, on a shoal, or bar, that existed in the river opposite the yards of the Commissioners, and the shoal being very much complained of, it was removed by dredging, and the river straightened." To get rid of a bar which was forming very rapidly at the north pier end and causing much inconvenience, Mr. Murray devised a floating boom, below which was suspended a canvas web, kept down by weights, the effect of which was to divert the channel and concentrate the force of the tide on the sand. The accumulated sand, however, driven from one spot, formed a bar at another. But though unsuccessful in this, Mr. Murray was more fortunate in accomplishing a work of considerable importance higher up the river, whereby a projecting rock in front of the Hetton and Lambton Coal Staiths, which had long been a great hindrance to navigation, was removed.

Mr. Murray's name is identified with one of the strangest operations recorded in the history of lighthouses. The eastern, or seaward portion of the North Pier, having fallen into a dilapidated condition, a new pier was built in place of it, to the east of the lighthouse, and into deeper water. The old pier and the lighthouse which stood upon it were consequently rendered useless, and preparations were made for the demolition of the latter. Mr. Murray, however, conceived the idea of transporting it in one solid piece to the site on which it had been intended to build the new lighthouse, a distance of more than 150 yards. The project was a bold one, for at that time the public mind had not become accustomed to the removal of large buildings from one site to another. The lighthouse was 69 feet high, and 15 feet in diameter at the base; its weight was 757,120 lbs.; and the new pier was 19 inches higher than the old, whilst its direction was entirely different. A number of openings were made in the base of the tower, by which the latter was supported on a solid platform of oaken planks, whilst a framework of stays or props, strengthened by crossbeams, surrounded the tower from base to summit. The platform rested on one hundred and forty-four cast-iron wheels, flanged like those of a locomotive, and running on eight parallel rails, which, with their "sleepers," were laid along the masonry of the pier and jetty. On the morning of the 2nd of August, 1841, the work of removal was commenced. After the mass had been moved a few feet, the rails were lifted and laid down again in front of the machinery, and this was repeated until the new site had been reached. A body of labourers worked windlasses on which were wound the chains attached to the platform. The first section of the work, 28 feet from south to north, was the heaviest, and involved the labour of forty men for about seven hours; whilst eighteen men were able in about thirteen and a half hours, to carry it over the second section, in a line of 447 feet from west to east. The operation of moving occupied a little over twenty working hours, and the lamp was lighted every evening during the progress of the operations, so that interference with the navigation was avoided. This achievement immediately established Mr. Murray's reputation as a bold and skilful engineer. He was awarded a Telford Medal of gold for a Paper on the subject presented to the Institution,[1] and also took medals at the Exhibitions in Paris, 1855, and in London, 1862, for models showing how the removal was accomplished. A slab let into the masonry of the pier indicates the site on which the lighthouse formerly stood.

The rapid growth of the trade of the port and the larger class of vessels frequenting it, soon made it apparent that if Sunderland were to hold its own, ampler provision would have to be made for the accommodation of shipping. It became clearer year by year that however much the river might be improved, the resources of the harbour would have to be supplemented by wet docks. Projects for the construction of docks had been put forward by Dodds in 1794, by Jessop in 1807, and by Robert Stevenson of Edinburgh in 1829; but nothing had come out of them. In 1831 meetings of the shipowners, merchants, and other inhabitants of the town were held, to urge upon the River Wear Commissioners the necessity of undertaking this work, and in the same year Giles and Brunel prepared designs for docks on both sides of the river, near its mouth, but these plans were rejected by Parliament. Mr. George Rennie and Mr. Walker were in the following year called in to plan small docks for each side, to be capable of extension. They recommended, for the site on the north shore, the low ground between the sea and the cliff, and on the south side the Fort, part of the Town Moor, part of the Commissioners' own ground, and extending to the shore. The Commissioners deposited these plans, but abandoned the North Dock project, in consequence of a desire on the part of the original proprietors to carry out the work themselves, and they were eventually obliged to withdraw the South Dock plans also, through being served with notice of an injunction from the Hetton Coal Company and others. The North Dock was begun in 1835, and completed some three

---

[1] Minutes of Proceedings Inst. C.E., vol. iii., p. 342. Also "Lighthouses and Lightships." By W. H. Davenport Adams, 1878.

years afterwards. The shipping community on the south side had, however, to wait a good many years before their wishes were realised.

In 1842 Mr. Murray brought forward his scheme for converting the Wear into a floating harbour. He had already, in 1832, submitted a similar project, in which he proposed to take advantage of a bend in the river at the ferry landing, and thus obtain an area of 24 acres. The navigable channel was to be diverted through the town of Monkwearmouth, nearly in a straight line from the bridge to the river mouth, by which an increased force of water would have been obtained on the channel and bar. The estimated cost of this earlier scheme was £200,000. The plan of 1842 differed in one or two important particulars from that of ten years before. A better acquaintance with the locality had convinced him that other means must be contrived for the passage of the river water than the formation of a cutting through Monkwearmouth, which would have been attended with enormous expense and difficulty. He therefore hit upon an idea, which up to that time had not been tried anywhere else, of providing navigable gates for the admission of the tidal waters. His scheme was, briefly, to convert the lower part of the river into a floating dock, by a wall of masonry, with a waste weir and sluicing gates, together with piers and navigable gates between them, running diagonally across the river from the High Coble Slip to the Folly End, and from that downwards to the Ham Sand, enclosing a tidal basin on the north side of the river. By this means 100 acres of water would be rendered available for shipping, producing at Pallion, about 3 miles from the river mouth, a depth of 5 feet in the channel, and in the vicinity of the gates an average depth of 12 feet. The gates were to be left open till three hours after high water, when the water on the outer side of the gates would ebb away, while it would be rising on the inner, or upper side. As soon as the penned-up water rose to the top of the gates the sluices would be opened, and the channel outwards would obtain the benefit of the scouring power of the liberated water. The gates were to remain closed till the tide should flow again to the level of the impounded water of three hours' flood, when the tidal water would force them open, and the navigation of the river proceed as usual. The cost of these works Mr. Murray estimated at £60,000, and he was of opinion that they could be carried out within twelve months, without interruption to the navigation. The scheme encountered a good deal of opposition, notwithstanding which it was adopted by the Commissioners; but ultimately it was abandoned, in consequence of the opposition of the Admiralty.

Undaunted by the failure of this last project, Mr. Murray submitted, in 1846, another scheme, upon his own initiative, for docks along the sea shore, with an outlet into the river at one end, and into the Hendon Bay at the other. His ideas were readily taken up, and under the auspices of the late George Hudson, the "Railway King," a private company was formed, in which many of the principal merchants and capitalists of the town interested themselves. The company obtained parliamentary powers, and tested the scheme practically by building three groynes running out seaward. These preliminary experiments having proved satisfactory, the directors proceeded with the work of making 18½ acres of the northern portion of the dock and the tidal basin communicating with the river. As it was not their intention at that time to make the sea-outlet, Mr. Murray designed a half tide basin, as an addition to the dock, in order to pass vessels in after the gates at the north end had been closed. Grave doubts had been expressed as to whether a south outlet would answer, and, in deference to these, after taking the advice of Mr. Rendel, Past-President Inst. C.E., the company had decided to postpone this work for a time. It was, however, subsequently carried out, after careful examination of Hendon Bay, in a more easterly direction than had originally been intended. If Mr. Murray was fertile in design, he was equally energetic in execution, for, once begun, the dock works were pushed on vigorously, so that within a little over three years after first submitting his plans they (with the exception of the sea-outlet section) were consummated. The foundation stone was laid on the 4th of February, 1848, and the Great Dock, with its basins communicating with the river, was opened on the 20th of June, 1850. The south outlet and graving dock were opened in the course of the year 1856. The docks remained in the possession of the company until August 1859, when they were transferred to the re-constituted River Wear Commission, under whose control they have since been greatly extended. A new dock of considerable area has been added, and two years ago a sea-lock was opened, by which vessels are enabled to leave or enter at any state of the tide. It is almost unnecessary to refer to the influence these docks have exerted upon the trade of Sunderland. If their construction had been delayed but a few years, Sunderland would have sunk into a third- or fourth-rate port, frequented only by the smaller class of shipping. The splendid docks, which owe their creation to the skill of Mr. Murray, have alone enabled it to cope with the altered conditions under which the carrying trade of the world is now conducted.

On the formation of the Dock Company, Mr. Murray became their engineer, and left the service of the Commissioners. About the same time he commenced practice in London, from which he paid visits to Sunderland at frequent intervals, retaining his appointment with the Dock Company until that body ceased to exist. He removed to Queen Square, Westminster, and entered into practice as a consulting engineer, his advice being largely sought in connection with harbour- and dock-works. The busy, middle period of his professional life now gave place to the practice of the committee-room and the arbitration-court, and he found leisure to take a leading part in the proceedings of the Institution, of which he had been elected a Member on the 12th of March, 1833. In December 1859 Mr. Murray was elected to a seat on the Council, which he retained until 1871. Besides his duties as a member of the governing body, he was active in imparting information from his well-stored mind, at the weekly meetings. On all subjects relating to docks, harbours, the maintenance of rivers, and the arrest of destructive coast-action, his opinions were received with attention and respect. He presented four original communications to the Society, three of which, being read before his appearance at the Council-board, were rewarded with medals and premiums. The first of these was the Paper already alluded to, being an "Account of the Removal of the Lighthouse at Sunderland." Subsequently came "An Account of the progressive improvement of Sunderland Harbour and the River Wear" (vol. vi., p. 256); "On the progressive Construction of the Sunderland Docks" (vol. xv., p. 418); and an essay, "On the North Sea, with Remarks on some of its Firths and Estuaries" (vol. xx., p. 314), which largely dealt with questions relating to the tides, to which he had paid great attention. In the general Indexes to vols. i.–xxx. of the Minutes of Proceedings are not less than five columns of entries under Mr. Murray's name, evincing the interest he took in the Institution. When it is added that he was a zealous attendant at the Council meetings, and took his full share of all Committee-work, it will be recognised that the Society has good cause to honour the memory of so staunch and indefatigable a supporter.

About the year 1870 Mr. Murray's bodily powers began to fail, although the change was but slight, and his mental qualities remained unimpaired. Finding his practice leaving him, he resolved to retire and spend the evening of his days at Sutton, in

Surrey. The cessation from the strain of professional life seemed to benefit him, and for nearly ten years he led a life of lettered leisure, gradually sinking under the infirmities of age, till he died quietly and painlessly on the 2nd of February, 1882, in his seventy-eighth year.

MR. CHARLES GEORGE NAPIER, eldest son of the late Captain Henry Edward Napier, R.N., F.R.S., was born at Florence in 1829. He commenced his education for Civil Engineering at King's College, London, where he attended the lectures during 1848 and 1849. He then became pupil of Mr. G. W. Hemans, late Vice-President Inst. C.E., completing his pupilage in 1852. After being employed on various railway surveys till 1853, he was appointed Resident Engineer on a portion of the Longford branch of the Midland Great Western Railway (Ireland), then in progress. In 1855, these works being finished, he went to the Crimea as Assistant-Superintendent in the third division of the Army Works Corps. Coming home, at the conclusion of the war, he was engaged in various railway surveys till 1858, when he took charge of the survey and construction of the Athenry and Tuam Railway, as Chief Resident Engineer under Mr. Hemans, till 1860, when that line was opened for traffic. During this period he also conducted the Parliamentary survey of the Athenry and Ennis Junction Railway, and other works.

In 1861 he was engaged on the River Fergus Embankment and Reclamation Survey, and from the end of that year till 1864 was Chief Resident Engineer on the Enniskillen and Bundoran Railway, conducting the survey and superintending the construction of the line, besides other work. From December 1864 till the early part of 1871 he was Engineer (in charge) of the southern division of the Great Southern and Western Railway of Ireland, having care of the permanent way, works, and stations, of over nearly 200 miles of line. Here he was enabled to arrest and quell a serious strike among the navvies, which, commencing at the northern end of the line, was rapidly extending, and which, but for the prompt and firm action taken by him on that occasion, must have proved disastrous.

From 1871 to 1874 he engaged in private practice in Dublin and London. At this latter date he was appointed (on the nomination of Mr. Gregory, Past-President Inst. C.E.) a Resident Engineer on the Cape Government railways in South Africa, where he carried out surveys and the construction of a very difficult portion of those lines, returning home in 1877 on the completion of his term of agreement. He was engaged in 1879 in preparing plans and estimates for the arterial drainage of an extensive district in the counties of Galway and Roscommon, and in the following year (1880-1) he was employed by the Commissioners of Public Works (Ireland) as an Inspecting Engineer of Baronial Relief Works, and gave much satisfaction for the manner in which he performed that duty.

During a professional career of upwards of thirty years, Mr. Napier made many friends, and was held in great esteem for his high engineering acquirements, tact, judgment, and soundness of views and opinions, as well as for his unswerving probity and rectitude of character.

He was author of a Paper on permanent way (written for the Institution of Civil Engineers of Ireland), and also of a pamphlet advocating the connection of the various railway stations at Cork by a central terminus. He was elected a Member of the Institution of Civil Engineers on the 7th of February, 1871, and was also a Member of the Irish Institution, a Fellow of the Geological Society, and a Member of the Athenæum Club. He died on the 2nd of September, 1882.

MR. CUTHBERT KNIGHTLEY ORLEBAR, the eldest son of the Rev. Cuthbert Orlebar, was born in 1844. He was educated at Brighton College, and on the death of his father, in 1861, entered on a pupilage of three years to Mr. G. W. Hemans, M. Inst. C.E. He was employed both in London and in Ireland in the office and the fieldwork, and in works in progress. For three years (1864-7) he was employed on the Enniskillen and Bundoran Railway, for two years as an Assistant, and for the last year as the Resident, Engineer. Here he devoted himself zealously to his work; the wild lake-scenery, and the rough surroundings of his life, possessed a charm for him equalling that of a sea-life to many men. He availed himself of every opportunity of gaining a knowledge of his professional work, and of designs to be brought before Parliament. Having won Mr. Hemans' approval and confidence, he was next appointed as Resident Engineer of a moiety of the Athenry and Ennis Railway from 1867 to 1869. He made contract surveys for the Wexford and New Ross Railway, as well as Parliamentary and contract surveys for railways, water, and sewage works, both for Mr. Hemans and for Sir Charles Fox, Mr. Hassard, Messrs. Wilkinson, and others. For three and a half years, from 1870 to 1873, he was Resident Engineer of the Whitchurch and Tattenhall Railway, under Mr. W. Clarke, M. Inst. C.E. He afterwards engaged in independent practice, and designed and executed amongst many other matters, new waterworks for the town of Conway, railways in South Wales, and Parliamentary surveys.

Previously to his appointment to the Ennis and Athenry Railway in 1867, he had passed a most successful examination for India; Colonel Chesney pressed upon him an important appointment, but he was warned that India would be prejudicial to his health. An appointment in Honduras was similarly offered, and ultimately declined on the same account. His marriage in 1873 brought with it a large accession of fortune, but he always retained his keen interest in his profession, nor did he in any way remit his really hard work in preparation of designs, estimates, and drawings, and of Parliamentary work.

Of late years his health, never strong, gave way, but with characteristic reserve he kept the illness a complete secret, even from his nearest friends. He was elected a Member of the Institution on the 10th of April, 1877, and died at Torquay on the 23rd of January, 1882, aged thirty-seven, after what seemed only a sudden attack. Mr. Orlebar's natural reticence was equalled by his fine sense of honour, his kindness of heart, and his aiming at perfection in all he undertook to accomplish. The charm of his character drew round him many friends, who have keenly felt his loss.

MR. THOMAS ORMISTON, C.I.E., was born in Edinburgh on the 28th of July, 1826; a few years subsequently his parents removed to Glasgow, where he received the rudiments of his education. He left school early, and, evincing an inclination and talent for engineering, he entered the service of his father and his uncle, who were then engaged in extensive building operations at the west end of Glasgow. Here he acquired a practical knowledge of carpentry, masonry, and surveying.

In the year 1846 he entered the service of the trustees of the River Clyde Navigation in the engineer's department, and shortly after became chief assistant to the then Engineer, Mr. David Bremner, M. Inst. C.E., and during the long illness, and for some time after the death, of Mr. Bremner in 1852, he had the entire charge of the works. After the appointment of Mr. John F. Ure, M. Inst, C.E., to succeed Mr. Bremner, Mr. Ormiston continued as chief assistant until the beginning of 1855, when Mr. James Walker, Past-President Inst. C.E., then the Consulting Engineer to the River Clyde Trustees, having had frequent opportunity of observing Mr. Ormiston's energy and ability, appointed him principal assistant in the office of Messrs. Walker, Burges, and Cooper, in London.

During Mr. Ormiston's engagement on the River Clyde he acquired a large and varied experience, as the great works of improvement, which have made that river famous in the annals of river-engineering, were then in full operation, and afforded scope for his abilities, which were highly appreciated by the Trustees and the Chief Engineer. He devoted much attention to the improvement of the dredging-plant, and in 1854 he furnished the designs for a dredger for the Ayr Harbour Trustees, which is still at work.

Mr. Ormiston continued in the service of Messrs. Walker, Burges, and Cooper, for about seven years, until the beginning of 1862, and during this period he frequently accompanied the late Mr. Walker to the many important works upon which he was engaged to report, amongst others the improvement of the harbours in the Isle of Man, the River Mersey, the extension of the Bute Docks, Cardiff, and many of the lighthouses for the Honourable Corporation of Trinity House. He was also entrusted with the entire charge of the erection of the lighthouse on the Needles Rock during 1856-7. No contractor being employed, he designed the whole of the necessary plant, and carried out the works to the entire satisfaction of Mr. Walker and the Trinity Corporation, and received a testimonial from the latter on the completion of the works.

Having been engaged under Messrs. Walker, Burges, and Cooper, in the preparation of the designs and contract drawings for the foundation of the Plymouth Breakwater Fort, the contract for which was let to Messrs. Henry Lee and Son, Mr. Ormiston, whose energetic nature led him always to prefer the active direction of works, accepted from Messrs. Lee the appointment as their Engineer in sole charge of the execution of the Fort works. These he continued to direct until near their completion in October 1864, when he received the appointment of Chief Engineer to the Elphinstone Land and Press Company of Bombay, in which city he arrived in the following December.

The Elphinstone Land and Press Company was formed in 1858 for the purpose of reclaiming a large extent of land from the foreshore of the harbour of Bombay, the formation of a series of tidal basins for native craft and boats, the construction of warehouses, roads, &c.

The portion of foreshore to be reclaimed presented at low water an expanse of soft mud 1 mile long and ½ mile wide, the effluvium from which was a constant source of danger to the adjoining densely populated native town, whilst from the boatmen frequenting this portion of the foreshore cholera was never absent.

Although the works had been commenced in 1859, only about 81 acres had been reclaimed when Mr. Ormiston took charge in January 1865. He immediately reorganised the establishment, appointed a proper staff, and reduced the number of workmen by two-thirds, and, having got the works fairly in hand, operations proceeded much more rapidly and at considerably less cost than previously.

By the year 1870 this foreshore, 328 acres in extent, had been reclaimed and converted into a valuable estate, 10,000,000 tons of material having been absorbed, the average depth of filling being 16½ feet; 9 miles of roads from 40 feet to 80 feet wide; 10 miles of drains and 2 miles of permanent sea walls had been constructed, affording basins for the native craft, with 70 acres of wharf space and extensive shed and warehouse accommodation. Of the reclaimed land, 84 acres were handed over to Government for a goods station for the Great Indian Peninsula Railway, and 100 acres were set apart as a land estate, the building-plots on which are at the present time rapidly being occupied. Space was also provided for the expansion of the overcrowded native town, and the medical authorities testify to the incalculable benefit of the works to the sanitary condition of Bombay, cholera being now stamped out at the Bunders, which have taken the place of the old foreshore landing-places.

The Government, being alive to the importance of these works, purchased the Elphinstone estate in April 1870, and took over the services of Mr. Ormiston as Engineer, transferring to his charge the reclamation works, which had for some time been in progress under Government in Mody Bay, and shortly afterwards the Government acquired the remaining foreshore properties.

This led up to and eventuated in the formation by Government of the Bombay Port Trust in 1873, which took over the administration of the entire harbour of Bombay, superseding the previously existing Harbour and Pilotage Board, Mr. Ormiston being appointed Chief Engineer.

After the constitution of the Port Trust, Mr. Ormiston having for some time previously become convinced of the necessity for improved appliances to meet the increasing trade, began persistently to advocate the construction of a wet-dock with the most modern hydraulic appliances. His proposal for a considerable time met with a cold reception, but his customary perseverance at length overcame all opposition, and he had the satisfaction of receiving the sanction of Government to his designs. In July 1875 orders were given to proceed with the works of this the first wet dock of any extent in India. The contract was let to Messrs. Glover and Company, and the first stone of the Princes Dock was laid by His Royal Highness the Prince of Wales on the 11th of November, 1875; the last stone was set, and the water admitted, on the 10th of April, 1879, and the dock was finally opened for traffic on the 1st of January, 1880.

The Princes Dock has an area of 30 acres of water space, with two entrances from the harbour, 66 feet and 55 feet wide respectively, having a depth of 28 feet of water on the sills at ordinary spring tides. Its construction involved the erection of 1½ mile of dock-wall, 37 feet in height, a large extent of sheds and warehouses, and a complete equipment of hydraulic machinery for loading and discharging vessels of the largest class. There are forty-three portable hydraulic cranes on the quays for lifts of 30 cwt., and one crane capable of lifting 100 tons. In opening up the channels and approaches about 4 million tons of dredging were executed with plant designed by Mr. Ormiston.

The success of the dock is universally admitted, and already the question of an extension of dock accommodation is engaging the attention of the Port Trust, as it is acknowledged that but for facilities afforded by the Princes Dock it would have been impossible to have dealt with the very large trade which has passed through Bombay during the last two years. The dock was also recently found to be of very great advantage to Government in enabling the expeditionary force to be despatched to Egypt in the middle of the monsoon with promptness and economy.

The rents from the land estate are increasing rapidly, and ultimately, from this source alone, the whole cost of the dock estate and its management will be defrayed, and the trustees will be enabled to reduce the port dues, and so attract a still larger trade to Bombay. It was with this view that Mr. Ormiston strongly opposed the separation of the land estate from the dock estate, and the wisdom of his policy has been fully established.

The amount expended on works in Bombay under the direction of Mr. Ormiston was about three millions sterling, and amongst many other works, in addition to the reclamation and dock works which he carried out, may be mentioned the Prongs Lighthouse, a tower 150 feet high on a dangerous reef, at the entrance of the harbour of Bombay. He designed the lighthouse in course of erection on the Sunk Rock near the harbour, and erected numerous beacons and landmarks in the harbour, also the overbridges for the road traffic between the native town and the reclamations, in lieu of the dangerous level crossings on the Great Indian Peninsula Railway.

Mr. Ormiston was for many years a justice of the peace for Bombay, and as a member of the corporation he took a keen interest in all municipal matters; he was consulted on all the drainage and waterworks projects, to the consideration of which he devoted much labour and time, for the most part gratuitously. He was entrusted with many arbitration cases for government and individuals, and frequently sat as an assessor on engineering questions before the courts. He was consulted on harbour and other works in different parts of India, and designed the Albert Edward breakwater now in course of construction, under native engineers, to form a harbour for the town of Mandvi, on the coast of Kattywar; he also designed a breakwater for Verawal, on the same coast, and was consulted by Government with regard to Kurrachee harbour, reported on the water power of the River Nerbudda at Bhosawal, and made many reports concerning the development of the internal resources of the country. In 1879 he visited Cyprus at the request of the Foreign Office, and prepared a report and design for a harbour at Famagusta.

Mr. Ormiston relinquished the post of chief Resident Engineer in 1877 to his brother Mr. George E. Ormiston, M. Inst. C.E., and became Consulting Engineer to the Bombay Port Trust in London, involving a yearly visit to Bombay. His last visit to India was to attend the opening of the Princes Dock in January 1880, on which occasion he received the decoration of Companion of the Indian Empire, in recognition of his public services.

He was a Fellow of the University of Bombay, and was elected Dean of Faculty of Engineering in 1879; he was also President of the Sassoon Mechanics' Institution, into which he infused much new vigour.

In 1880 his health began to give way, but in spite of much bodily weakness he continued his work almost to the end, and as late as February 1881 he visited Venezuela in South America, in his capacity as Chairman of the Bolivar Railway Company; and, with that thoroughness which characterised him, he minutely inspected the whole of the line and introduced such changes in the management as he considered desirable. He was, however, obliged to give up all work early in 1882, and proceeded to the Isle of Wight in the hope of regaining strength; but, unfortunately, his weakness increased, and he died at Freshwater on the 9th July, 1882, at the comparatively early age of fifty-six.

He was elected a Member of the Institution on the 28th of May, 1861, and at all times took an active interest in its affairs. He was zealous for the honour of his profession, and was much respected by all classes for his frank and honest character, while his indomitable energy and perseverance, professional ability and sterling integrity inspired confidence in all works he undertook. He was always ready to assist a junior member of the profession by his advice, and his generosity of heart and just and kind treatment of the large number of workmen at various times under his control, gave him an unusual influence over the native workmen of Bombay. The announcement of his death was received with universal regret in Bombay, and an influential committee was appointed at a public meeting, largely attended by native gentlemen, to take steps to commemorate the services he rendered to that city.

MR. WILLIAM POWELL was born at the village of Hoyland, near Barnsley, in 1824. When little more than a boy he was placed under Mr. John Hawthorn, engineer and manager of the Milton Ironworks, near Sheffield, where he is said to have been first employed at one of the rolling-mills, but was afterwards promoted to the drawing-office. Leaving Milton in 1846, he removed to Rotherham, and was employed on the staff of the late Mr. Charles Bartholomew, engineer to the South Yorkshire Railway, and in the service of that gentleman ample scope was afforded him of acquiring practical knowledge of the profession, which he availed himself of to the utmost extent. In 1854 he left Rotherham to occupy a position of great responsibility as engineering assistant to Mr. John Towlerton Leather, contractor, upon the great Breakwater works at Portland. In the construction of this important national undertaking he was engaged about a dozen years. During the conduct of the work he came much into contact with Mr. (now Sir John) Coode, M. Inst. C.E., who so appreciated his services that he offered him the appointment of Resident, or Superintending Engineer, on the works then about to be commenced by the Government of the Isle of Man. From 1867 to 1879 Mr. Powell was employed in the construction of the breakwater at Port Erin; the new landing-pier, breakwater, and quays at Douglas; on the harbour works at Ramsay, Peel, and other places in the island, and of the sea promenade at Douglas. The Weymouth Waterworks were constructed under his supervision.

Mr. Powell was a thoroughly trustworthy practical engineer, of unblemished character, and held in esteem by all who knew him. He was elected an Associate of the Institution on the 1st of March, 1870, and transferred to Member on the 13th of February, 1872. He died on the 22nd of May, 1882.

MR. MIDDLETON RAYNE was born at Rayne's Park, near Kingston-on-Thames, on the 1st November, 1830. It was assumed he would devote himself to farming, and after an excellent early training in the school of Mr. Whitehead at Ramsgate, he entered the Hoddesdon Training School (Herts), where he took the first prize, a gold medal, for general proficiency.

He afterwards developed a great taste for mechanics and engineering, and in 1858 he determined to make engineering his profession. In 1859 he went to India on the staff of the Great Southern of India Railway in a subordinate position, and worked his way, under Mr. G. B. Bruce and Mr. M. W. Carr (MM. Inst. C.E.), to an appointment as Executive Engineer in 1863. He was then given charge of a district, and remained on the Great Southern of India Railway for seven years in all. The works of that railway were executed departmentally, and Mr. Rayne had engineering and executive charge of the construction of bridges over the Noyel and Ambrawatty rivers. These bridges have an aggregate of 1,300 feet of waterway, and are works of some importance.

In 1866 Mr. Rayne—employed for that purpose by Messrs. Waring Brothers and Hunt—examined the route of the then projected Indus Valley Railway and reported in detail thereon. He was then specially employed by the same firm to erect the ironwork of the girder bridges on their Jubbulpore contract. This work had some features of novelty and demanded judgment, skill and energy in no ordinary degree for its successful accomplishment. Spread over a distance of some 200 miles, through jungle without roads, numerous gaps had to be spanned by girders, in twelve months. Among many other openings, were fourteen of 110 feet span each and 40 to 50 feet high. The peculiarity of the work lay in the method to be adopted in order to economise time without incurring extravagant cost. The larger girders were for the most part erected complete, in pairs with cross-girders attached, on the embankment clear of the bridge and launched into position by a method designed and executed by Mr. Rayne for this work, and explained by him in an interesting Paper published in 1867 among "Professional Papers on Indian Engineering."

In 1868 he went to the Punjâb as Superintending Engineer on the Public Works Establishment of India. In this capacity and sometimes as officiating Chief-Engineer, he was occupied for eleven years in the construction of the Punjâb Northern and Indus Valley State Railways departmentally. Among the more important works in the construction of which he was most intimately connected, were the bridges over the rivers Jhelum,[1] Sutlej, and Chenab. The Lower Sutlej bridge was especially his; it has sixteen openings, each spanned by 250-feet girders, the piers resting each on three brick wells of 18 feet 9 inches outside and 8 feet 9 inches inside diameter, sunk, with their curbs, to a depth of 103 feet 6 inches below lowest water-level. The Chenab or Alexandra bridge,[2] only a trifle under 2 miles in length, was opened by the Prince of Wales in January 1876.

When in 1879 the Indian Government determined to curtail their railway operations, and offered extraordinary inducements to the senior members of their Public Works Establishment to retire, Mr. Rayne, among others, returned to England, and was not actively engaged in the pursuit of his profession afterwards.

He had been suffering from cold and an affection of the throat for some days, but nothing serious was suspected until about forty-eight hours before his death, which occurred on the 9th of October last from inflammation of the brain.

During an active professional career extending over twenty years he had made many friends, and his comparatively early death will be deeply regretted by very many who knew him as an agreeable and accomplished companion, a true and loyal friend. By those who knew him most intimately his memory will be held especially dear for his eminently amiable and generous qualities.

Mr. Rayne was elected an Associate of the Institution in January 1868, and was transferred to the class of Members in November 1874.

---

[1] Minutes of Proceedings Inst. C.E., vol. liv., p. 94.
[2] *Ibid.*, p. 71.

Mr. HENRY THOMAS TANNER was born at Speenhamland, Berkshire, on the 21st of August, 1842. He was a student in the Applied Sciences Department, King's College, London, and was also specially taught mechanical drawing by Mr. W. Bains, Assoc. Inst. C.E., before being articled, in 1860, for three years to Mr. T. O. Donaldson, M. Inst. C.E. On the completion of his pupilage he was for five years employed in general engineering work, mostly as Assistant to various members of the Institution, and in different parts of Europe. The numerous engagements thus filled gave him a greater insight into the practical part of his profession than is usually obtained at so early an age, and paved the way for his obtaining an appointment in the Indian Public Works Department as an engineer under covenant for five years. He landed in Bombay in December 1868, and was posted to the office of the Superintendent of the Sirhind Canal, Colonel (now Major-General) H. W. Gulliver, R.E. Having successfully passed the Lower and Departmental standards in Hindoostanee, he was promoted to the rank of Executive Engineer, fourth grade, in 1871, and transferred to the first division of the canal in July of that year. A few months later he was posted to the office of the Joint Secretary to Government, Punjab Irrigation Branch, under Colonel (now Major-General) Crofton, R.E., and chosen to officiate as Assistant Secretary under that officer. While holding the office his covenant expired, but on the recommendation of the Local Government his services were retained, and in March 1875 he was promoted to Executive Engineer, third grade. About a year later, Mr. Tanner was transferred first to the (late) Special Survey Division, Derajat Circle, and secondly to the fifth Division of the Sirhind Canal, both changes occurring within six months. After eighteen months furlough he returned to the Sirhind Canal, taking over the executive charge of the third Division at Loodianah from Mr. J. C. Hammer, and was shortly after promoted to Executive Engineer, second grade. He held this charge until his sudden and premature death, on the 24th of December, 1881, from the bursting of a blood-vessel.

Mr. Tanner was an able engineer and a zealous hard-working officer, much liked by those who came in contact with him. Colonel Swinton, R.E., the Superintending Engineer of the Sirhind Canal, writes, "By Mr. Tanner's death the Government has lost a very useful public servant, and his friends and fellow-workers have lost a very pleasant companion." He joined the Institution as an Associate on the 5th of February, 1867, and was transferred to the class of Member on the 7th of January, 1879.

MR. JOHN SCOTT TUCKER, the son of a Naval Surveyor in the service of the Admiralty, was born about the year 1814. He was educated under Dr. Burney at Greenwich, and in 1830 was articled for five years to Sir John Rennie, Past-President Inst. C.E., serving his time at the well-known works in Upper Ground Street, Blackfriars. During his pupilage he was employed for some time on the construction of the new London Bridge. In 1835 he became assistant to Messrs. Walker and Burges, MM. Inst. C.E., for whom he made an elaborate survey of Reculver, and the shore defences at that place, with a view to the acquirement by the Trinity House for a sea-mark of the celebrated ruins of Reculver church. On leaving Messrs. Walker and Burges he was again for some time employed by Sir John Rennie, and was also associated with the late Mr. James Cooper, M. Inst. C.E., in surveying the metropolitan section of the Northern and Eastern (now Great Eastern Railway). He also assisted Mr. (now Sir John) Hawkshaw, Past-President Inst. C.E., in making the test-levels between London and Horsham for the original London and Brighton Railway scheme. This second engagement under Sir John Rennie lasted only two years, but was a busy and exciting era in Mr. Tucker's career, the memorable struggle of the rival schemes for a line to Brighton being at its height. He was also at the Azores, and there surveyed the harbour of Ponte del Garda. In 1839–40 Mr. Tucker was engaged on the Great Western Railway under Mr. Brunel, Vice-President Inst. C.E. About 1841 he went to Bermuda as Clerk of Works for the Admiralty during the building of the docks, and on their completion resumed his engagement under Mr. Brunel, being employed on the Cornwall Railway. The next few years were passed in general engineering work, during which he made another visit to the Azores. From 1848 to 1851 Mr. Tucker was in the service of the Admiralty in charge of naval works at Malta. He then went to South Africa to report on a proposed railway at Cape Town, after which he removed to South America and became Resident Engineer of the Pernambuco Railway, but only remained in Brazil for a year.

In July 1858 Sir George Grey, Governor of the Cape, submitted to the Secretary of State the opinion of himself and the Executive Council, that "It would be very desirable that the Council of the Institution of Civil Engineers should be requested to select from the names of candidates such person as they may think best qualified to fill the duties required from the Colonial Engineer." The nomination was accordingly entrusted to the Institution, and

Mr. Scott Tucker was unanimously chosen from among a limited competition of five, the Council being so satisfied with his professional and other qualifications that they did not think it necessary to invite applications from other candidates. Unfortunately, however, long mismanagement had brought the engineer-department at the Cape into thorough disrepute, and Mr. Scott Tucker found himself, from the moment of taking up his appointment, in a very awkward and delicate position. This unsatisfactory state of things continued until a Commission of Inquiry was issued to report on the condition of the public works of the colony. In giving evidence before this body Mr. Tucker, being a man of high honour and despairing of any improvement under the existing organisation, recommended the abolition of the office he himself held. In the result this advice was taken, and in 1873 Mr. Scott Tucker retired on a pension. It is right to add that the Duke of Newcastle, the Home Secretary, put on record the following minute:—

"I am bound to say that I have observed nothing in any part of the evidence taken before the committees of the two Houses of the Cape Parliament to reflect upon Mr. Tucker's character, and it is with much regret that I have felt myself obliged to acquiesce in his removal from an office of which he seems to have been called upon to discharge the duties under very disadvantageous circumstances.

"His case is one which ought to be regarded by the colonial authorities as entitling him to much consideration."

After this Mr. Scott Tucker had an office in Westminster, and was occupied in various works, principally as a consulting engineer, and he served under the British Commission for the Paris Exhibition of 1867; but to use his own words, the broken nature of his previous employment had put him out of the engineering groove at home, and the stream of professional success flowed past him. His claims to government employment, however, procured him the post of superintendent of Public Works for the Island of Barbadoes, to which he was appointed in 1876. The appointment carried with it a commissionership of lighthouses, and a membership of the Board of Health. He was also made a J.P. of the island. But the climate did not agree with him, and after about four years' service he retired. Compelled to escape from a tropical climate, he landed in England in the midst of the severe winter of 1879–80, and never recovered the shock, although he lingered for more than two years. He died at Dover on the 18th of January, 1882, in his sixty-seventh year.

Mr. Scott Tucker was an amiable and accomplished gentleman; but he was not endowed with those qualities which lead to high success, apparently lacking stedfastness of character. He was very apt at the draughtsman's board; while in the office of Messrs. Walker and Burges, he made reduced copies of the contract drawings for Dover Harbour with such skill of execution that they were bound and presented to the Duke of Wellington, who took great interest in that work. But Mr. Tucker was more than a mere draughtsman, being in fact a skilled artist in water colours. He made many very beautiful perspectives of the old Blackfriars and Westminster bridges, and of lighthouses for the Trinity House, and was an exhibitor at the Society of British Artists in 1836, contributing a water-colour entitled "Trefusis Mill." He also produced some vivid sketches of Maltese life and character seen from an English point of view. In 1861, when at the Cape, he organised a corps of Engineer Volunteers, of which he was gazetted Lieutenant-colonel.

He was elected an Associate of the Institution on the 22nd of January, 1833, his certificate bearing the signature of Thomas Telford as chairman of the meeting at which his name was passed; subsequently he was transferred to the class of Graduate, and was one of the six who alone remained in that grade when it was abolished in 1867. He was then, on the 26th of November of that year, transferred to the class of Member, for which he was supposed to have qualified some thirty years before.

Mr. HENRY ALTY was born in the year 1844, and was educated by the Rev. Canon Fergie, B.D., Vicar of Ince and Honorary Canon of Liverpool, when headmaster of the Wigan Blue-coat School, where Alty was highly commended for his industry and amiability. Mr. Richard Alty, the father, died when his son was only fourteen years of age. On the recommendation of Canon Fergie the son commenced his professional career, in September 1858, as a pupil of the late Mr. John Law Hunter, M. Inst. C.E., then engineer to the Wigan Corporation, in whose office Alty remained till 1865, when he was appointed Assistant-Surveyor to the Halifax Corporation. After this, and previous to his appointment as Borough Engineer and Surveyor at Plymouth, Mr. Alty held the positions of Engineer to the Keighley Local Board of Health, and Borough Surveyor at Barrow-in-Furness. On entering upon his duties at Plymouth, Mr. Alty displayed, for one of his years, considerable ability and tact in dealing with corporation business and work generally. Quiet of manner, yet firm of purpose, and of a practical disposition, he commanded the respect of all who had either business or professional dealings with him. Although during the three years of his tenure of office (December 1878 to March 1882) no important works were executed by the Plymouth Corporation, Mr. Alty had much to do in initiating and negotiating for extensive improvements now about to be undertaken.

Always of a delicate constitution, a conscientious and sensitive temperament, his work told on him, and indeed his early decease was doubtless accelerated by a too constant application to professional duties.

The Plymouth Corporation required designs for a new market, costing about £22,000, and into this competition, conjointly with Mr. Charles King, architect, Mr. Alty threw himself with, as the sequel proved, too much energy. The fact of his successful efforts in this competition was announced two or three days after his untimely death, which thus deprived the profession, at the early age of thirty-eight years, of a thoroughly trained, skilful, and progressive borough-engineer.

Mr. Alty was elected an Associate Member of the Institution on the 6th of December, 1881.

MR. WILLIAM SCOTT HENDERSON, the second son of Mr. Thomas Henderson, artist-engraver, was born in London on the 6th of February, 1844. In 1860 he was articled for three years to Mr. T. J. Hill, of Finsbury, architect and surveyor. On the completion of his pupilage he was for a short time engaged by Mr. Danby, of Parliament Street, Westminster, and then went to Hungary, as assistant for Messrs. Warings, the contractors for the Eastern Railway in that country; but left in 1870, in consequence of the suspension of the works. He then filled a similar position in the extension of the Somerset and Dorset Railway to Bath, for Messrs. T. and C. Walker. After which he was for a short time in Mexico, for Messrs. G. B. Crawley and Co. In 1874 he went to Brazil on a three years' engagement with Messrs. Edwin Clark and Punchard, who had contracted to make the line from Recife to Limoeiro, now known as the Great Western Railway of Brazil; but bad health compelled him to leave before the expiry of his agreement. After a short period at home to dissipate the ill effects of a tropical climate, Mr. Henderson proceeded to the Cape of Good Hope as assistant to Mr. J. A. Kendrew, Assoc. Inst. C.E., the agent for Mr. W. F. Faviell, who had undertaken to construct the Cape Government Railways. He was at first employed on the Midland line from Port Elizabeth to Graaf Reinet, and on its completion in 1879, took charge of the North-Eastern line, terminating at Cradock. He had just completed the works in this district, and was preparing to return home, when inflammation of the spinal membrane, probably induced by the sudden changes of temperature prevalent in that place at certain times of the year, prostrated him, and he died on the 9th of August, 1881, after three days of great suffering.

Mr. Henderson had excellent ability and good general professional knowledge. He was an unusually energetic, hard-working man, and in every respect a thoroughly satisfactory agent. He was much esteemed at Cradock, and a large concourse of the inhabitants assembled at his grave in the English churchyard at that place. He was elected an Associate Member of the Institution on the 16th of January, 1877.

COLONEL JOHN THOMAS SMITH, R.E., F.R.S., who died on the 14th of May, 1882, was one of the oldest Associates of the Institution, having been elected on the 23rd of February, 1836. He was the second son of Mr. George Smith, of Edwalton, Notts, and afterwards of Foëlallt, Cardiganshire, and was born in or about the year 1805. After receiving his early education at Repton, he proceeded to Addiscombe, where, passing out first in mathematics, he obtained a commission in the Engineers, and in 1825 left for India. Having been appointed Executive Engineer in the north of the Madras Presidency, he took up the question of limes and cements, and translated Vicat's standard treatise on the subject. Being a good practical chemist, he was able to enrich that valuable work by many original investigations of his own, added in the form of notes. Soon after this he was called upon to arrange a system of lights for the South Indian coast, and in 1838 the present lighthouse at Madras was erected from his designs, and furnished with a "reciprocating light," invented by him to suit the peculiar locality.

Upon Colonel Smith's return to England in 1837, his labours in the field of practical engineering science were recognised by his election as a Fellow of the Royal Society. He had previously been elected President of the Philosophical Society of Edinburgh.

He soon went back to India, and, at the request of the Government, set to work to reorganise the Madras Mint, introducing steam-machinery and establishing such a system that he abolished the usual allowance for "waste" of the precious metals—a result of considerable importance to the Government. At this time he devised many ingenious mechanical arrangements, among them an automatic weighing and assorting machine for blanks of coins, based upon the principle of the hydrostatic balance, which has been described, by Sir Arthur Cotton, as "one of the most beautiful specimens of mechanical ingenuity that ever was invented."[1] During his stay in Madras Colonel Smith originated, and for some years edited, the Professional Papers of the Madras Engineers, and himself contributed a large number of Papers upon various engineering subjects.

After being several years in charge of the Madras Mint, he was appointed to a similar duty at Calcutta; but he soon retired from the service, receiving the thanks of the Government of India for his "scientific skill and exertions."

Upon returning to England, Colonel Smith was for a time consulting engineer to some Indian irrigation companies; then he became a director, and eventually chairman, of the Madras Railway Company—a position which he held during the remainder of his life. He was also actively at work in other ways. The Government employed him to advise as to mint machinery and other subjects, and to investigate questions connected with the Madras Military Fund. In conjunction with Professor Graham, F.R.S., he reported upon mintage, and he attended as the British representative at the Monetary Conference held in Paris in 1865; besides which, he was an earnest and active member of the Committee of the Church Missionary Society.

For many years Colonel Smith had been occupied in considering the great questions connected with the depreciation of silver in India and the Indian exchanges—bi-metallism, &c. His long study of questions of Political Economy, and his clear and powerful mind, well qualified him to undertake the investigation of intricate problems of this nature. Foreseeing a great loss to the Indian exchequer by the depreciation in the value of silver, and feeling sure that he could devise a scheme to prevent that loss—which, up to the present time, has amounted to several millions sterling—he published his views in 1876, and strongly urged them—answering all objectors in subsequent publications. Political economists

---

[1] The Royal Engineers' Journal, July 1st, 1882.

of the highest order expressed to him their approval of the course of action he proposed. Though the question has been forced aside by more pressing matters, it may be that some day his views may be carried out, effecting a saving of many millions, and conferring an enormous benefit upon the country in whose interest alone he so earnestly laboured.

Colonel Smith was elected an Associate of Council of the Institution of Civil Engineers, and served in the year 1877. He was a member of the (Danish) Society of Northern Antiquaries, and of various other societies. He was besides the author of several scientific works and papers.

During a long and active life Colonel Smith's characteristic was a rigid adherence to a high standard of duty, and thoroughness in carrying out all that he undertook. In private life his clear and vigorous mind, and his deep sympathy with all who were in need or trouble, caused him to be widely sought as a friend and counsellor. He was a truly humble Christian man, respected by all who knew him, and beloved by a large family and circle of brother officers and friends.

MAJOR **WILLIAM SWAINSON SUART**, R.E., second son of the late Mr. Edward Suart, of Lancaster, was born near Liverpool on the 30th of July, 1814. In 1832 he entered the Military College of the East Indian Company at Addiscombe, and passed out at the end of the following year as Senior Cadet. In the final examination he took, with one exception, all the prizes, including the sword for good conduct. After leaving the Military College he passed a year at Chatham in the School of Military Engineering. On proceeding to India in 1836 he became Assistant to the Civil Engineer and Architect in charge of all the Government public buildings in the Fort Town and Island of Bombay, and was also engaged on reclamation works in North Concan. Two years later he selected and surveyed the western half of the road to Nagpore, and the postal route from Bombay to Calcutta. From 1839 to 1843 Lieut. Suart was Engineer to the municipality of Bombay, during which time he had charge of the roads, drains, buildings, and of the town and island, and designed and erected new sluices for the drainage of that part below high-water mark. He also laid out new streets and roads, with the necessary bridges. Besides his municipal duties, he acted as Engineer to a company of merchants in reclaiming land from the sea at Colaba, making a landing-pier and designing and erecting thereon large warehouses. He also built the Bombay Theatre and the Byculla Club. In 1839 he was appointed Assistant Engineer of the Bombay Mint, and during the tenure of that office superintended the repairs to the machinery of Government steamers. In December 1848 he was transferred to Aden as Executive Engineer, being promoted to Commanding Engineer in April 1851. He remained there for six years, erecting forts. He retired from the East India Company's service, with the rank of major, in November 1857.

On his return to England, Major Suart settled at Chigwell, in Essex, when he became a J.P., and took an active part in local affairs. He was Chairman of the Governors of the Chigwell Grammar School, and was mainly instrumental in raising it from a school of under twenty boys to one of upwards of a hundred and fifty. He was also churchwarden of Chigwell for twenty-three years, Chairman of the Sewer Authority of the Commission of Dagenham Level, of the visiting justices of Ilford Gaol, and served on the committee of Essex Lunatic Asylum, as well as many other local institutions. He was most regular in his attendance to all magisterial duties, and his clear perception and strictly fair construction of all matters, gained him the respect and esteem of all with whom he came in contact. In addition to his heavy local work, he was engaged in the direction and management of several public companies. His somewhat premature death on the 23rd of May, 1882, was the result of overwork from his multifarious duties, public and private.

Major Suart was elected an Associate of the Institution on the 9th of January, 1872.

SECT. III.

ABSTRACTS OF PAPERS IN FOREIGN TRANSACTIONS AND PERIODICALS.

On the Measurement of Wind-Pressure—Incompleteness of Borda's Formula. By DR. FINES.

(Comptes rendus de l'Association Française pour l'avancement des Sciences, 1881, p. 457.)

Borda's formula for deducing pressure of wind from velocity is $F = C \times SV^2$.

C being a constant coefficient for a certain plate, S the surface in square metres, V the velocity of the wind in metres per second, F the pressure of the wind in kilograms per square metre. C varies with size and shape of plate. Borda gives the following values:—

C = 0·092	kilogram	for plain plates of	0·011664	square metre
0·100	”	”	0·026244	”
0·104	”	”	0·059049	”
0·120	”	”	0·929000	”

The first three of the above values are given by Borda, the last by Rousse. Later Navier found that in the case of movement in the atmosphere at 10° Centigrade, under the normal pressure $C = 0 \cdot 1278$. Thibault, however, found the value of C under the same conditions to be 0·1151. Dupré considers that Navier's formula, $R = 0 \cdot 1278\ SV^2$, is the right one to use.

The Author states that Borda's formula gives rise to two sources of error, one owing to inaccuracy in the formula, the other to its application. Though the size and shape of the plate are known, the reciprocal reactions of the different currents of air one upon the other are not; neither are their absolute velocities, the mean velocity only is known.

The ordinary anemometer is incapable of recording the velocity of the wind at the time (of only a few seconds' duration) of the most violent gusts. Mr. Renard, however, made observations during the violent storm of Nov. 16, 1880. He found that during an interval of twenty-five seconds the velocity increased from 38 to 51 metres per second, and then diminished in the following twenty seconds to 35·70 metres, so that during this short interval the pressure varied from 325 to 136 kilograms per square metre. These observations, however, do not give the absolute maximum intensity which can have lasted only a few seconds, and may have been much higher than that above stated, and it is the absolute maximum velocity which is required for an exact application of Borda's formula.

W. H. T.

The Strain-Indicator. By Dr. W. Fränkel.

(Der Civilingenieur, 1882, p. 191.)

The Author describes in this Paper his improved strain-indicator (*Dehnungszeiger*). This is an instrument which can be attached to a bar, and by measuring and recording the extension or compression between two points affords a clue to the stress in the bar.

The apparatus consists of a tube, one end of which is fastened to the bar under examination by a clip; the other end is free to slide in a frame which is also attached to the bar; the relative motion of the end of the tube and the frame gives the strain in the part of the bar lying between the two clips.

The motion is rendered visible, and its amount recorded, by means of multiplying levers, whose ratio is 200 to 1; these levers move a pencil across a ribbon of paper travelled by clockwork. The recording mechanism can be thrown in or out of gear, either directly by a finger key, or by an electro-magnet worked from any convenient position. Direct micrometric measurements show that the motion of the pencil is proportional to the alteration in length of the bar, with but a very slight error. Various examples are given in the Paper of the diagrams drawn by the instrument when applied to various parts of structures and machines.

The results obtained by attaching the instrument to the members of a wrought-iron railway bridge during the passage of a train are discussed at length; they showed that the calculated stresses practically agreed with those really arising in the members. It is worthy of note that the examined bars did not return to their normal position after the passage of a train until another train had passed in an opposite direction.

W. P.

On the Effect of Prolonged Stress upon the Strength and Elasticity of Pine Timber.[1]

By R. H. Thurston, of Hoboken, N.J.

(Proceedings of the American Association for the Advancement of Science, 1881, p. 9.)

The experiments of Mr. Herman Haupt, made forty years ago, proved that the length of time during which timber is subjected to a stress has a very considerable effect on the resulting strain, and that consequently, in determining the breaking-load, it is necessary to consider the case of a prolonged stress.

Using pieces of good selected timber, 60 × 3 × 1 inches, set as cantilevers, with a breaking-moment due to a load of 48 inch-pounds, he obtained for the value of $R = \frac{6.w.l}{b\,d^2}$ the following figures:—

Wood.	R.	Time.	Remarks.
White pine . . .	2272	10 minutes.	Injured.
,, ,, . . .	1548	16 days.	,,
Hemlock	2624	5 minutes.	,,
,, ...	1620	16 days.	,,
Yellow pine . . .	2848	5 minutes.	,,
,, ,, . . .	1800	16 days.	,,
Locust	5504	2 minutes.	Uninjured.
,, ...	3600	3½ days.	Injured.
,, ...	2304	16 days.	,,
White oak . . .	4248	16 minutes.	Uninjured.
,, ,, . . .	7200	15 minutes.	Injured.
,, ,, . . .	3648	40 hours.	Uninjured.
,, ,, . . .	4088	48 hours.	Injured.

The investigations of the Author, carried on in the Stevens Institute of Technology, relate only to yellow pine. A plank was selected, the history of which was known. The stick was cut at Jacksonville, Fla., in October, 1879; was received early in the following year, and air-seasoned, partly out-of-doors and partly in-doors for six months, until the spring of 1880, when the experiments were made.

A set of sticks were first cut from 40 to 54 inches long, and with a cross-section of from 1½ to 3 inches square. These were used for determining the moduli of elasticity. Taking the expression $R = \frac{3}{2}\frac{P.l}{b\,d^2}$, the moduli of rupture ranged from 11,000 to 12,000, and of elasticity from 2¼ millions. The specific gravity varied from 0·75 to 1·00, and increased but little when the wood was kiln-dried to a moderate extent; but the modulus of elasticity rose to 2½ millions, and the modulus of rupture increased about 20 per cent.

---

[1] This is an amplification of a Paper printed in the 'Journal of the Franklin Institute' for 1880, and abstracted in the Minutes of Proceedings Inst. C.E., vol. lxiii., p. 339.

From the unused part of the plank a set of three pieces were then cut, about 1 inch square in section, and tested on supports 40 inches apart in the usual way. The results are given in the following Table:—

Load.	A $b = 1·113$ in. $d = 1·105$ „	B $b = 1·107$ in. $c = 1·107$ „	C $b = 1·1$ in. $e = 1·1$ „	—
	Deflection.	Deflection.	Deflection.	
Lbs.	Inches.	Inches.	Inches.	
50	0·2127	0·2035	0·2188	
..	0·2164	0·2125	0·2231	After 5 minutes.
100	0·4339	0·3935	0·4528	
..	0·4378	0·4000	0·4623	,, ,,
200	0·8984	0·7640	0·9298	
..	0·9146	0·7730	0·9468	,, ,,
300	1·5109	1·1805	1·5648	
..	1·5931	1·2180	1·6713	,, ,,
..	1·6029	..	1·6883	After 6 minutes.
325	..	..	1·8568	
340	..	..	Splintered	
345	..	..	Broke	
350	1·9329	1·4705	..	
..	..	1·5381	..	After 5 minutes.
..	..	1·5700	..	After 6 minutes.
380	Broke	..	..	
410	..	Broke	..	

It is apparent from these figures that 375 lbs. is about the average breaking-load for the section taken, when the load is only momentarily applied.

Nine other pieces were then cut and dressed to the same section, and tested as before on supports 40 inches apart. The three sets were loaded with a constant load of 350, 300, and 250 lbs. respectively, and the deflection observed at successive intervals of time.

I.—LOAD, 350 lbs.

A $b = 1·1$; $d = 1·1$ inch.		B $b = 1·12$; $d = 1·12$ inch.		C $b = 1·1$; $d = 1·1$ inch.	
Time Load was applied.	Deflection.	Time Load was applied.	Deflection.	Time Load was applied.	Deflection.
Hours.	Inches.	Hours.	Inch.	Hours.	Inches.
..	0·1565	..	0·1705	..	0·1840
..	1·7350	..	1·7175	..	2·0300
18	2·3385	Between 27 & 30½	Broke	¼	2·3500
43	Broke	..	..	Between 4½ & 13½	Broke

II.—LOAD, 300 lbs.

A $b = 1 \cdot 1$; $d = 1 \cdot 08$ inch.		B $b = 1 \cdot 1$; $d = 1 \cdot 12$ inch.		C $b = 1 \cdot 1$; $d = 1 \cdot 12$ inch.	
Time Load was applied.	Deflection.	Time Load was applied.	Deflection.	Time Load was applied.	Deflection.
Hours.	Inches.	Hours.	Inches.	Hours.	Inches.
1	1·5980	½	1·1835	1	1·8660
3	1·6666	3	1·2370	2	1·8950
5	1·7316	5	1·2770	18½	2·2450
22½	1·9171	21¾	1·4280	44	2·5450
70½	2·2596	69¾	1·6380	66½	3·0000
78½	2·4796	77¾	1·7740	{Between 79½ & 88½}	Broke
118½	3·0586	117¾	1·9360	..	..
121	Broke	310	2·0630	..	..
..	..	646	2·6600	..	..
..	..	719	Broke	..	..

III.—LOAD, 250 lbs.

A $b = 1 \cdot 08$; $d = 1 \cdot 1$ inch.		B $b = 1 \cdot 08$; $d = 1 \cdot 1$ inch.		C $b = 1 \cdot 1$; $d = 1 \cdot 1$ inch.	
Time Load was applied.	Deflection.	Time Load was applied.	Deflection.	Time Load was applied.	Deflection.
Hours.	Inches.	Hours.	Inches.	Hours.	Inches.
91	1·2927	90	1·3757	89	1·2696
210½	1·4237	209½	1·5592	208½	1·4246
353½	1·5657	352½	1·7492	351½	1·6036
523	1·6257	522	1·8132	521	1·6636
618	1·7417	617	1·9522	616	1·7646
810	1·8827	809	2·1032	888	1·9026
2,923	2·2757	2,922	2·4917	6,713	2·6416
8,899	Broke	6,066	Broke	11,100	Broke

On comparing these Tables it is seen that the whole set of bars, loaded with 87½ per cent. of the maximum load obtained by the usual tests broke within two days. In the second set, with a load of 75 per cent. of the maximum, there is greater divergence in the time of breaking; but this is probably due to the differences of strength more than to variations of the effect of time of stress. With the last set, where the load was 60 per cent. of the maximum, the increase of deflection was almost precisely the same for all the bars for several months—a fact which shows the gradual progress and steadiness of yielding, and that no accident produced the final rupture. After several months, the piece which had shown most pliability broke down. The second piece, intermediate in stiffness between the two others, broke at the end of one year; whereas the last piece slowly yielded, and finally broke at the end of fifteen months.

Comparing the ultimate deflection attained by the several bars, it is seen that the average, under ordinary tests, was about 1·8 inch. Under a load 95 per cent. of that then carried, the extreme deflection was 2·4 inches; and with loads of 80 per cent. and 70 per cent., 3 inches and a little under 3 inches, respectively.

The last set of observations shows that a load of 60 per cent. of the maximum is unsafe, although possibly a smaller load might have been carried indefinitely.

Taking the probable breaking-load under unintermitted static stress as 50 per cent. of the maximum given by the usual tests, and then applying a factor of safety of 2, a safe factor is obtained, based on the ordinary test, of 4. But to make allowance for uncertainties as to the character of material, and for sudden shakes and impacts, the Author considers that the factor of safety should not be less than 8.

E. H.

The Fatigue of Small Spruce Beams. By F. E. Kidder.

(Journal of the Franklin Institute, October, 1882, p. 261.)

The object of the experiments here recorded was to determine what proportion of the so-called transverse breaking-load of a beam would, under the most favourable conditions, ultimately cause the beam to break. The beams were cut from two spruce planks, which had been in a drying-kiln for three weeks. They were straight-grained, free from knots, 1½ inch square, placed on supports 40 inches apart, and loaded at the centre.

In one series of experiments, a load of 50 lbs., about one-fifteenth of its breaking-load, was laid on one of the beams, with the object of determining whether the deflection would be stationary or variable. The deflection increased very rapidly for the first twenty-four hours, and then quite regularly, but slowly, for one hundred and ninety-two hours; after that time it decreased for seventy-two hours, when it slightly increased again. Whilst the deflection decreased, the weather was very wet. The initial deflection was 0·0803 inch, and the maximum deflection was 0·0975 inch. The piece was removed, and, at the end of several days of rest, it was tested with a load of 574 lbs., or three-fourths of the calculated breaking-load. The initial deflection was 1·0044 inches, and the ultimate deflection at the end of two hundred and sixty hours was 1·6721 inches, when the beam broke. Another beam broke with two-thirds of the calculated breaking-weight. Another beam was tested with 374 lbs., or one-half its breaking-weight, for three hundred and twenty-seven hours, during which time the deflection constantly increased. The load was removed, and the set of the beam gradually decreased, until it nearly disappeared. The beam was again tested, when the load was alternately laid on and removed. Each time the load was applied the deflection was increased, and the set increased much faster than the deflection. It was inferred that the beam would ultimately have broken under this treatment, if it had been continued.

From these and other results of tests, the Author concludes that the strength of spruce beams of small section, cut from seasoned timber, is not materially increased by the timber being kiln-dried; that the strongest beams bend the most before breaking; that under a load of from one-half to seven-eighths of the breaking-weight, the deflection increases rapidly for a few hours, then gradually, then again rapidly before breaking; that a load of one-half the breaking-weight, applied for a few days only, does not injure the beam; that a load which deflects a beam one-half of its maximum deflection before breaking, will ultimately break the beam; that one-half of the breaking-weight cannot be permanently supported by a beam. The position of the annular rings in spruce beams of small section materially affects the strength, which is least when the rings make an angle of 45° with the top and bottom surface, of the beam. The writer agrees with Professor Thurston that 5 is the lowest factor of safety that should be used for wooden beams under an absolutely static load.

There are several Tables and diagrams of deflection in the Paper.

D. K. C.

Experiments on the Hardwoods of Australia.

Compiled by F. A. CAMPBELL.

(Proceedings of the Royal Society of Victoria, 11 May, 1882.)

Premising that the useful hardwoods of Australia belong almost exclusively to the Eucalypt family, whence some difficulty in affixing true names to the timbers procured for experiment, the Author gives a short account of various tests, from those of the Sydney Mint in 1858 to Campbell's in 1879. He considers Mr. Laslett's work, "Timber and Timber Trees,"[1] to be the most valuable treatise on the subject, but points out an error in Laslett's results for tensile strength, arising from his having used pieces 7 feet long, but only 6 feet between the bearings, although, in calculating the values of E and S, elasticity and strength, he has unfortunately taken the length as 7 feet. Observing this discrepancy, the Author has calculated afresh the values of E and S for all cases to which he has found it necessary to refer to Laslett's work.

---

[1] "Timber, and Timber Trees, Native and Foreign." By Thomas Laslett, Timber Inspector to the Admiralty. London. Macmillan & Co. 1875. Can be seen in the Library of the Inst. C.E.

The results of the various experiments are combined in the following Table:—

TABLE.

Species of Timber.	Specific Gravity.	Crushing Weight per Square Inch.		E_1 from formula $E=\frac{l^3 W}{16 a d^3 \delta}$.	Modulus of Rupture.	Tensile Strength per Square Inch.
		Longitudinal.	Transverse.			
1. Iron bark (*Eucalyptus Leucoxylon and E. Siderophyloia*)	1·117	10,166	4,100	488,066	18,258	15,950
2. Tuart (*E. Gomphocephala*) .	1·169	9,340	..	447,700	13,890	10,284
3. Blackbutt (*E. Pilularis*) .	0·990	8,449	8,064	313,600	13,529	..
4. Bluegum (*E. Globulus*) . .	1·017	7,730	6,600	509,750	13,140	20,100
5. Yellow Box (*E. Melliodora*)	1·017	..	..	472,605	12,312	..
6. Bloodwood (*E. Corymbosa*) .	0·918	..	..	399,450	11,970	..
7. Spotted gum (*E. Goniocalya*)	0·981	9,072	7,308	322,900	11,943	..
8. Stringy-bark (*E. Macrorrhyncha* . . . . . .)	0·995	7,744	6,650	231,850	11,656	22,000
9. Kari (*E. Diversicolor*) . .	0·980	12,513	..	568,220	11,640	7,070
10. Woolybutt (*E. Longifolia*) .	1·054	7,297	2,968	285,995	11,524	..
11. Redgum (*E. Rostrata*) . .	0·990	..	..	433,000	10,250	16,400
12. Jarrah (*E. Marginata*) . .	1·007	7,166	..	177,690	9,250	2,940

(N.B. The compiler gives an additional column for names of his authorities—here omitted.)

The Author has also compiled the following Table of "safe practical moduli of rupture," for six of the principal hardwoods of the group, as follows:—

Timber.	Moduli of Rupture. lbs.
Ironbark	16,000
Bluegum	11,000
Yellow box	10,000
Spotted gum	10,000
Stringy bark	9,000
Redgum	8,000

F. G. D.

The Testing and Properties of Slow-setting or Portland Cements. By — BARREAU.

(Annales des Ponts et Chaussées, August, 1882, p. 150.)

The Author reviews the different methods of manufacturing and testing Portland cement. Attention is directed to the Papers of Mr. J. Grant,[1] many of whose tables are reproduced in full. Several analyses of English, French, and Belgian cements

---

[1] Minutes of Proceedings Inst. C.E., vols. xxv., p. 66, and xxxii., p. 266.

are given, the chemical composition of which appears to differ but slightly. The oxide of iron, sometimes present in considerable proportions in cement, is regarded as an inert substance; while the magnesia, in which some German cements are rich, is supposed to act in the same manner as lime when exposed to the action of water. Sulphuric acid, present as sulphate of lime, retards setting, and should not occur in appreciable quantities. Chemical analysis is considered indispensable for checking the composition of the cement before burning; the habit of relying upon practical tests alone, even if carried out by a skilled workman, being severely condemned. Fine grinding of the burnt cement is of the highest importance from an economical point of view. The coarse grains are of no more value than sand during the hardening process, and as it has been found that their composition is the same as the finer and more active particles, it follows that it is only possible to obtain a thoroughly good, energetic cement by fine grinding. The question of cost must, however, be taken into consideration. The cement should be passed through dressing-machines, and that which they reject should be re-ground. Although storing the cement may be advantageous when it contains free lime, it is not considered necessary with a cement free from this defect, but in practice it has been found that cements used fresh do occasionally give unsatisfactory results, while with stored cements no such bad results have occurred. In France the net weight of a cask of cement varies from 170 to 190 kilograms (375 to 419 lbs.), while the sacks contain 50 kilograms (110 lbs.). In Germany the official net weight is 170 kilograms (375 lbs.) for a cask, and 60 kilograms (132 lbs.) gross, for a sack.

A chemical test of Portland cement can rarely be carried out in practice, on account of the special skill required, but a test is proposed for ascertaining the amount of free lime in a cement. A weighed quantity of the sample is placed on a sieve, and water poured over it. The liquid is caught in a graduated tube, to the bottom of which the fine particles of cement sink, while the free lime is said to float above the cement as a kind of "laitance."[1] In the specification for the cement used in the Boulogne harbour works, the maximum of sulphate of lime was fixed at 1 per cent. A table is given showing the composition of particles of the same cement of different degrees of fineness. The fine and coarse particles appear almost identical. Vicat's needle is still used in France for determining the time of setting. At Boulogne the weight affixed to the needle was 300 grams (10·5 ozs.), while at Dunkirk 1·530 kilograms (3·37 lbs.) was considered necessary. Salt water was found to retard the setting of cement with which it was mixed, and a low temperature had the same effect. The crushing-

---

[1] This test is founded on the assumption that the "laitance" from Portland cement consists of free lime, whereas its analysis indicates that alumina and silica are its chief constituents, free lime being soluble in an excess of water.—W. F. R.

strength of cement can rarely be ascertained accurately, on account of the difficulty of making the briquettes perfectly regular, and of ensuring absolute contact. It has therefore been found advisable to use the tensile test as a criterion of the quality of cement. In France the briquettes are usually made of neat cement, and at Boulogne were tested at two separate intervals of forty-eight hours and one hundred and twenty hours. Greater importance is attached to the second test, which is the equivalent of the English seven-day test. The breaking-section of the briquette is 0·04 metre × 0·04 metre (1·57 inch × 1·57 inch). Uniformity of temperature is insisted upon in the room where the briquettes are kept during the hardening process. A number of tests were made at Boulogne from 1878–1880 with different mixtures of fine or coarse sand, with varying proportions of cement. In each case the coarse sand made the stronger mortar. A graphic representation of these tests shows that in many cases there was a diminution in the strength of the mortar between eighteen and twenty-one months, after which the increase continued again.

In an appendix to this Paper the specification for the cement for Boulogne harbour is given. The residue on a sieve of 180 meshes per lineal decimetre (45·7 meshes per lineal inch) must not exceed 10 per cent. of the volume tested. Any sample which, at a temperature of 15° Centigrade (59° Fahrenheit), when mixed with sea-water will support, after the lapse of half an hour, a needle with a point of 1 millimetre square (0·00155 square inch), weighted with 300 grams (10·5 ozs.), is to be rejected. The average tensile strength of the briquettes, after forty-eight hours' immersion in sea-water of 10° to 15° Centigrade, is to be 7½ kilograms per centimetre square (106·6 lbs. per square inch), and in one hundred and twenty hours, 12½ kilograms per centimetre square (177·8 lbs. per square inch).

W. F. R.

Experiments on Arches of Brickwork and Portland Cement.

By — DE PERRODIL.

(Annales des Ponts et Chaussées, August 1882, p. 111.)

The experiments detailed in this Paper were undertaken at the instance of the French Minister of Public Works, with the object of comparing the figures resulting from actual experiment with those obtained by calculation. The bricks used for the experimental arch measured 0·22 metre (8·6 inches) × 0·105 metre (4 inches), × 0·065 metre (2·5 inches), their crushing-strength varying from 39·34 kilograms per square centimetre (558 lbs. per square inch), to 165·75 kilograms per square centimetre (2,352 lbs. per square inch. The cement mortar used in the construction of the arch was composed of equal volumes of cement and sand. The cement weighed 1,380 kilograms per cubic metre (110·6 lbs. per bushel), in its loose state, and 1,920 kilograms per cubic metre (153·5 lbs. per bushel) when well shaken down. The weight of a cubic metre of the moist sand was exactly the same as that of the cement, so that one cubic metre of the mortar contained 952 kilograms of cement, and the same weight of sand. The crushing-strength of the cement-mortar, tested in cubes of 5 and 10 centimetres (2 and 4 inches), was 73·70 kilograms per square centimetre in seven days, and 93·4 kilograms per square centimetre in thirteen days.

The span of the arch was 20 metres (65½ feet), and its height 2 metres (6¼ feet), its thickness being the width of a brick, or about 4 inches. Two arches were constructed; but both of them appear to have been injured in striking the centering. The experiments were carried out by weighting the key of the arch and its sides, either simultaneously or alternately, and the deflections obtained are recorded in a series of Tables, which are, however, unsuited for an abstract.

W. F. R.

On the Production of Cement from Slags. By L. Roth.

(Stahl and Eisen, vol. ii., 1882, p. 488.)

The Author points out that Portland cement, and the slags obtained in the production of foundry pig-iron with coke, are essentially similar in qualitative composition, as is seen by the following analyses:—

	Cement.	Slag.
Lime	60·05	51·62
Silica	24·31	35·12
Alumina	7·50	9·53
Magnesia	1·17	1·58
Ferric oxide	3·34	..
Ferrous oxide	..	0·87
Manganous oxide	..	0·37
Potash	0·80	..
Soda	0·74	..
Sulphur	..	0·88
Gypsum	1·82	..
	99·73	99·97

In the slag, sulphur exists as sulphide of calcium, which would be injurious to the setting-power of the cement, and there is also a quantitative deficiency in certain bases. In order to supply these, the Author mixes the slag with Bauxite and limestone, or lime, and moulds the powder into bricks, which are burnt in an ordinary cement-kiln, Hofmann's annular kiln being preferred. In firing, the heat is raised gradually from dull to bright redness, to drive off combined water and carbonic acid, and to convert a portion of the sulphide of calcium into sulphuretted hydrogen, and aluminate of lime; after which it is clinkered at a strong white heat, and the resulting mass is crushed and ground in the usual way.

As the value of Bauxite depends upon its contents of free alumina, the proportion added will vary with its composition; the limits between which the finished product may be regarded as giving good cement are:—

Lime	55 to 63 per cent.
Silica	22 „ 26 „
Alumina	6 „ 10 „

The proportions used by the Author, with a basic slag of the composition previously given, which falls to powder when cooled, are:—100 slag, 85 limestone or chalk, with $CaCO_3$, 98, and SiO_2 2 per cent., Bauxite 15. The latter is the variety found near Giessen, which contains Al_2O_3, 48·5; Fe_2O_3, 13·52; SiO_2, 9·40 per cent.

This mixture when burnt gave 158·66 parts of cement, of the following composition:—

	Per Cent.
Lime	61·9
Silica	24·1
Alumina	10·6
Ferric oxide	1·3
Ferrous and manganous oxides	0·8
Magnesia	1·0
Sulphur	0·3
	100·0

One half of the total quantity of the sulphur in the slag was eliminated as sulphuretted hydrogen. The cement, when ground and sifted through a sieve having 900 holes to the inch, was of a greenish-grey colour, and of strong hydraulic properties, showing no tendency to swell when once set. It is essential to the process that the materials should be perfectly mixed before firing. Slags that fall to pieces in the air should be sifted through a 900-hole sieve, but when they are not sufficiently basic they should be granulated in water when run from the furnace, and ground in the cement-mill. A small addition of soda may be made advantageously in some cases, but as a rule it is not necessary. The cement may be applied in the formation of articles in concrete with granulated slag, which possess a certain hydraulic character.

H. B.

A New Paving-Material.

(Deutsche Bauzeitung, 1882, p. 485.)

The paving consists of bricks, 8 inches by 4 by 4, laid in hot tar on a bed of concrete, 6 inches in depth. In order to arrive at reliable conclusions as to the behaviour of this paving-material, it was laid down at the junction of the Leipzigerstrasse and Charlottenstrasse in Berlin, a spot over which the heaviest traffic passed; this was estimated at one thousand vehicles per hour, besides one thousand three hundred tramcars daily. The material appears to answer its purpose admirably. Granite, compressed asphalt, and wood pavement had been previously tried on the same spot, and had all required repair after having been down three months.

The numerous attempts hitherto made to render bricks tough and endue them with a high factor of resistance against pressure, by steeping or boiling them in tar, have all failed, because the bitumen is unable to penetrate to a sufficient depth. In this case, however, the bricks are placed in a vacuous chamber, hot asphalt being afterwards introduced. By this means the brick is freed from air and moisture, and absorbs from 15 to 20 per cent. of bitumen; it becomes very tough and elastic, is capable of withstanding great pressure, and absorbs no moisture.

These bricks have also been used for damp-proof courses, stable-pavements, retaining-walls, &c.

J. R. B.

Sub-aqueous Underpinning. By A. G. Menocal.

(Transactions of the American Society of Civil Engineers, June 1882, p. 181.)

The quay-wall of the Gosport Navy-yard was constructed between the years 1835 and 1842 of granite ashlar on a timber platform, supported on four rows of piles, at a depth of 18 feet below the coping, and 13 feet 4 inches below high-water level. Owing to subsequent dredging in the channel, and the removal of mud in front of the wall, a heavy settlement of the wall had in recent years taken place. An examination of the foundation by divers showed that the material beneath the timber platform had been carried away, undermining nearly the entire thickness of the wall for a considerable length. The platform and the heads of the piles being thus left exposed had been partially destroyed by the teredo, leaving the wall supported chiefly by the two back rows of piles, assisted by the bond of the masonry. In the hopes of arresting the settlement, a row of 18-inch piles were driven close together in contact with the outer edge of the foundation, but proved ineffectual, and the wall continued to settle rapidly. By this time the wall had at the worst point moved forward 10 inches, and had sunk vertically 18 inches. Owing to the difficulty in obtaining the necessary funds to rebuild the wall, it was decided to underpin a length of 140 feet where the movement had been greatest.

This work was commenced at the centre by cutting away the new close-piling in front of the foundation for a length of 6 feet; two piles of the first and second rows, the heads of which had been destroyed by teredo, were then sawn off level with the ground, and the remnants of the platform removed. Upon the solid stumps of the piles, stringers 12 inches by 12 inches were bolted, and a platform of 6-inch planking laid while the masonry overhead was carefully shored. Two piles of the third row were then sawn off at ground-level, and a waling bolted to the fourth row at the back of the wall-foundation. A timber platform was then built on this, forming a second shelf at a higher level than the first, and extending to the back of the wall. Concrete in bags of not more than 2 cubic feet was then laid in and well rammed, thus forming a pier of the whole width of the wall, and 6 feet in length, resting on the pile foundation, which was out of reach of the teredo. The sections of the wall on each side were then treated in a similar manner by building piers under their centres, and the work repeated at the centres of the subdivisions, until the concrete piers were about 6 feet apart throughout the whole length under repair.

Before closing the gaps from end to end, the walings of contiguous piers were scarfed and bolted together as far as practicable; the platforms and concrete being then put in, a continuous support is provided throughout. During the progress of the work a settlement of nearly 8 inches took place, but has now ceased.

In removing the fragments of stone and timber from the stiff bed of clay under the wall, a powerful jet of water was employed, delivered from steam-pump and hose.

In sixteen months a length of 300 feet of wall has been underpinned at a cost of 125 dollars per lineal foot, while the estimated cost of rebuilding the wall on a new foundation, instead of underpinning, was 447 dollars per lineal foot.

A. T. A.

Report on Naini Tal Drainage and Protective Works.

By W. Willcocks, Exec. Eng.

(Professional Papers on Indian Engineering, June 1882.)

The damage done to Naini Tal by the heavy rains of 1879 and 1880 was very extensive. Ravines were scoured out, heavy landslips occurred, roads were obliterated, and houses washed away. The main causes of the damage were, the careless way in which water from roads and roofs had been turned into the ravines without proper provision for carrying it off, and the quarrying of stone in the ravines, which deprived them of their only support. In many of the ravines loose shale of a very porous character overlies soft clay-slates to a depth of 8 or 10 feet. Houses were built on platforms cut out of the hillside, and the excavated material thrown down on to the loose shale, thus disturbing its centre of gravity.

The ravines would have carried the water well enough had it been discharged into them at the right places; but it was allowed to flow in high up, where it saturated the shale, which then was converted into a semi-fluid mass, and slid down the hill. This action continued year after year; the beds of the ravines were scoured out, roads carried away and replaced by others injudiciously cut in the hillsides, and the support of the hills was cut away to form platforms for houses. Ravines which ran semi-liquid shingle for three months in the year were banked across; and houses built in their beds, while the fluid, nearly as heavy as molten lead, was left to find its way down the hillsides as best it could. The heavy rainfall of last September (32 inches in forty-eight hours) found a portion of Naini Tal in this state. The results were disastrous. The main object of the protective works was to prevent any more shingle from running. They were as follows: (*a*) All cracks were filled up and planted with grass. (*b*) All houses were provided with masonry drains connected with the nearest ravine. (*c*) Strong revetment walls were built at weak places, especially under springs. The foundations were taken down to the slate underlying the shale. (*d*) Every ravine was provided with a masonry drain from top to bottom. (*e*) At intervals of about 100 feet two side-walls, inclined to the drain, were built about 2 feet higher than the drains, to bring back any water which might overflow owing to local slips of earth. (*f*) Roads and cuttings near houses were provided with retaining walls wherever they were needed. (*g*) All slips were thickly planted with grass, trees, and shrubs. (*h*) All roads except one were drained outwards.

Besides these general protective works, others were undertaken at special places. Retaining-walls were 2 feet thick at top, with front batter of 1 in 4 and vertical back. The Author gives details of cost, and specification for masonry, together with drawings of the works.

W. H. T.

Reservoir of the Bergères Cross-roads near Paris.

(Annales des Travaux publics, August, 1882, p. 683, 3 woodcuts.)

The Suburban Water Company of Paris, which distributes water from the Seine to seven suburbs, has recently erected this reservoir to meet the requirements of a rapidly increasing population. For whereas the number of houses supplied by the company was one thousand four hundred and twenty-three in 1872, it had risen to four thousand three hundred and eighty-eight in 1881; and the yearly rate of increase, which was one hundred and eighty houses in 1873, amounted to five hundred and sixty-two in 1881. Up to 1881 the company had five reservoirs, having a total capacity of 9,600 cubic yards, but now it can store up 13,800 cubic yards. The company has been endeavouring to substitute the measurement by meter for the customary distribution through a gauge tap, having an orifice which allows the prescribed volume to flow out in the twenty-four hours. The total volume of water raised in 1881 was 2,310,000 cubic yards, and of the 1,579,000 cubic yards supplied to private houses, 1,177,000 cubic yards were furnished by meter. On the meter system, by which water can be obtained at any time and in any quantity, the amount of water drawn off varies considerably, being greatest at particular times of day, and in very hot weather. The meter system, however, whilst necessitating more pumping power and a greater reservoir capacity, is better for the consumer than the gauge system, as it dispenses with cumbrous cisterns liable to be injured by frost, and furnishes the water under pressure and in any desired quantity. The new reservoir is 102 feet long, and 87 feet in average width; the water surface is 14 feet 9 inches above the floor, and both the supply- and discharge-pipes have a diameter of 16 inches. The reservoir is covered over by eight semicircular brick arches, having each a span of 9 feet 10 inches, resting on arched piers, so that the vaulted roof rests on sixty pillars, each 2¼ feet square. The side walls are built of rubble masonry, and the floor is of concrete, 2 feet in thickness. The interior is covered with a coating of Boulogne cement mortar, consisting of equal parts of sand and cement, about 1¼ inch thick, laid on in three layers. The work was begun in June, and finished in October, 1881, but the reservoir was not filled till the middle of 1882, to allow the work to get thoroughly dry.

L. V. H.

On the Removal of Deposit from Reservoirs in Algeria.

By MARTIN CALMELS.

(Comptes rendus de l'Association Française pour l'avancement des Sciences, 1881, p. 243.)

The reservoirs in Algeria are of great size, and they are of immense importance to the prosperity of the country; after being constructed under great difficulties, and at considerable expense, they are in danger of being completely filled up, unless means are taken for the removal of the deposit which accumulates rapidly in them. For instance, the reservoir at Saint Denis du Sig, constructed with a capacity of 122,000,000 cubic feet, had in 1879 silted up to the extent of 24,000,000 cubic feet. The Habra reservoir,[1] constructed in 1871 with a capacity of 105,000,000 cubic feet, contained, in 1879, 70,000,000 cubic feet of deposit.

---

[1] The dam of this reservoir burst in December, 1881. See Minutes of Proceedings, Inst. C.E., vol. lxx., p. 447.—SEC. INST. C.E.

The same thing takes place, but to a less extent, in Spain, and the system adopted for cleaning the reservoirs in that country is to wash them out about once in every four years. This, however, not only necessitates the emptying of the reservoir, but also causes the loss of the sediment, which is generally very valuable for irrigation. The method of remedying the evil applied by the Author at Saint Denis du Sig, consists of blowing air into the sediment, thereby stirring it up and allowing it to run off with the water. The air may be compressed by means of turbines driven by the water as it passes out of the reservoir, conducted through india-rubber pipes over the dam, and carried down into the mud by an iron tube which is attached to a float, by means of which it can be moved from one part of the reservoir to another. The Author describes the apparatus in detail, and states that as the result of an experiment on a small scale (using a 12-HP. portable steam-engine for compressing the air), the water, clear at first, became rapidly charged with the stirred-up sediment to a distance of 110 feet round the pipe. It is only necessary to inject the air near the dam, for, as the mud is removed from this position, a fresh supply flows down from the more distant parts of the reservoir, and this is in its turn stirred up into the water in a similar manner. The Author estimates the cost of an apparatus for cleansing a reservoir of the capacity of 455,000,000 cubic feet at £2,600.

W. H. T.

On the Agricultural Utilisation of Road-scrapings and Town-Refuse. By A. Ladureau.

(Comptes rendus de l'Association Française pour l'avancement des Sciences, 1880, p. 1040.)

The Author states that the town of Lille is one in which the scavenging is most thoroughly done. The road-scrapings are greatly prized as fertilisers by agriculturists in the neighbourhood, and he has carefully examined into their true theoretical value, in order to foster the estimation in which they are held. It costs the local authority 2 francs 50 centimes to collect each cubic metre (about 2*s.* per ton), and to bring it to the depôt, and it is sold to farmers at 2 francs per metre, being a loss of 20 per cent., in order to ensure a ready sale. Sent away to a distance in railway trucks, the Author finds the total cost to be as follows:—

		F. C.	
To 10 kilometres distant,	transport costs	2·83	per cubic metre.
„ 20 „ „	„ „	3·18	„
„ 30 „ „	„ „	3·52	„
„ 40 „ „	„ „	3·87	„
„ 50 „ „	„ „	4·21	„
„ 60 „ „	„ „	4·55	„

As the result of analyses of old and recent samples of road-scrapings, he finds the theoretical values, taking the nitrogen at

2·50 francs the kilogram, phosphoric acid at 30 centimes, and potash at 80 centimes the kilogram, to be, in the former case, 6·25 francs the cubic metre or ton, and in the latter 5·60 francs. Detailed analyses of each kind of refuse are given.

The mud-deposit in sewers and drains was next analysed. This deposit contained from 80 to 90 per cent. of water, and was most foul and fetid. After partially drying it, till the water was reduced to 50 per cent., the residue contained nitrogen, 0·675; phosphoric acid, 0·81; potash, 0·18 per cent.; and the amounts were not greatly decreased when the deposit had been taken to a depôt and stored for some time, until it became partially air-dried.

An analysis follows of the sewage water of Lille, which is shown to be very dilute, and contains only about one-fifth of the impurities present in the Paris sewage. The latter contained 1,677 grams of solid matters in suspension, and 1,056 grams per cubic metre of dissolved impurities, while the total impurities present in the Lille sewage were, on an average of twenty samples, taken at different hours of the day, only 525 grams per metre.

The results of some experiments in treating the sewage of Roubaix and Tourcoing with lime and clay are then given. The sewage of these towns is greatly charged with the refuse from dye-works, and the foul water from wool-washing and combing establishments. Under the superintendence of Mr. de Mollins a number of trials of precipitation have been undertaken, and it has been found that in order to precipitate 1 cubic metre of sewage water of average quality, 1 kilogram of clay and 300 grams of quicklime are needed, costing about half a centime. As these two towns yield together about 20,000 cubic metres of sewage per diem, the cost of clarification would be, for the precipitants alone, without treatment, 100 francs per day, or 36,000 francs per annum. The water after such treatment would be pure enough for all practical purposes, but the solid residue, it is admitted, would be difficult to deal with. This deposit, after being carefully dried by means of gentle heat, contained in one hundred parts—

Water	18·00
Organic nitrogenous matter	4·37[1]
Organic matter non-nitrogenous	14·63
Raw fatty acid	10·00
Inorganic matter and mineral salts	53·00
	100·00

The cubic metre of the deposit would therefore yield 42 grams of nitrogen, and from 500 to 600 grams of fatty substances. It would scarcely be practicable to extract this small quantity of fatty matters at a profit, though by distillation in retorts they might be converted into illuminating gas. Such a deposit could

---

[1] Containing 0·70 nitrogen.

not be employed as a manure. The sewage water might, however, be employed for irrigating the sand dunes of Northern France and Belgium, and the Author suggests that the matter is worthy of the consideration of a company.

G. R. R.

Viaduct of La Chanca on the North Western Railway of Spain.

(Anales de Obras publicas, vol. iv., p. 5.)

This viaduct has a total length of 978 feet, and its maximum height is 95 feet. It contains twenty semicircular arches of 33 feet span, divided into four groups of five arches by three piers of double thickness. The thickness of the arches at the crown is 2½ feet. The single piers are 6½ feet thick at the springing of the arches, and have a batter of 1 in 50. The three double piers are 11½ feet thick at the springing and are strengthened with lateral counterforts, projecting 1 foot at the springing and battering 1 in 50. The width of the viaduct is 18 feet. The stone employed is principally a quartzose slate quarried in the immediate neighbourhood of the viaduct with granite quoins. The voussoirs are slates, 4 inches thick at the intrados, 27 to 30 inches broad, and about 3 feet 3 inches in length. Considerable difficulty was experienced in working these slates into the proper wedge shape for voussoirs. This operation alone cost 7*s.* 2½*d.* per cubic yard of voussoirs, a vast number of stones being spoilt. The quantities of the viaduct are : 302 cubic yards concrete in foundations, 15,990 cubic yards slate in rubble and voussoirs, 4,131 cubic yards granite ashlar. The total cost of construction was about £22,100. The time employed was two years, but the work was somewhat delayed by financial difficulties.

E. M.

Wood required by Railways in France. By E. VIGNES.

(La Nature, Paris, 23 September, 1882.)

In 1877 the six great railway companies of France used, for maintenance and renewals, two million five hundred and sixty-three thousand wooden sleepers, or ninety-three per kilometre (say one hundred and twenty-nine per mile), equal to a daily consumption of seven thousand. Supposing a tree to furnish ten sleepers—which is below the number for the beech, while above it for the oak—this involves the felling daily of seven hundred fine trees. Since 1877 the French network has been greatly extended, and when the scheme at present being carried out is complete, there will be required one thousand trees per day. To this great consumption must be added the wood wanted for the maintenance and repair of the rolling-stock, which was, about the time mentioned, 4,944,240 cubic feet per annum. It was also estimated that, between 1877 and 1892, the construction of 20,000 kilometres of new lines would absorb twenty millions of new sleepers. The Northern and Eastern railway companies laid ten sleepers under an 8-metre (26·24 feet) rail, and the tendency is to decrease the space between the sleepers, so as to increase the solidity of the track. From these facts it is inferred that railways are the greatest consumers of wood in a country; and the question of metallic permanent way becomes of growing importance.

F. G. D.

Tests of the Efficiency of various Fish-plates used on the North-Western Railway of Austria. By W. Hohenegger.

(Organ für die Fortschritte des Eisenbahnwesens, vol. xix., 1882, p. 181.)

The Author has compiled a Table of the results of tests made by him on rail-joints, fished with plates of various sections, with the object of determining the efficiency of the double-angled fish-plate, introduced some years ago on several German and Austrian railways. The ordinary, the single-angled, and the double-angled fish-plates are those employed; they are tested with regard both to vertical and lateral resistance. To ensure accuracy in the tests, the Author obtained the ordinary fish-plate by planing down the flange of the single-angled one. The rails used in the tests were of the section of the North-Western Railway of Austria, and were placed on sleepers 1·06 metre (3 feet 5¾ inches) from centre to centre. The Table shows that, in the case of a rail-joint fished with a single-angled and a double-angled plate, a pressure of 15 tons, acting vertically, produces a deflection of 9·5 millimetres (⅜ of an inch); whilst, in the case of a joint with a single-angled plate on each side of the rail, the same vertical pressure produces a deflection of 22 millimetres (⅞ of an inch). If, now, the pressure of 15 tons be applied, so as to act horizontally on the fish consisting of a single-angled and a double-angled plate, a deflection of 37·5 millimetres (1½ inch) is the result; and when two single-angled fish-plates are used, the deflection amounts to 60·5 millimetres (2⅜ inches).

J. R. B.

Statistics of Spanish Railways for 1881.

(Revista de Obras públicas, vol. xxx., p. 69.)

On the 1st of January, 1882, there existed in Spain 7,739 kilometres (4,524 miles) of railways open to traffic, and 1,937 kilometres (1,203 miles) in course of construction. During the year 1881, 155 miles of railway, and 5 miles of tramway, have been opened to traffic.

During the same year, the Spanish government have paid to different railways as subvention a total sum in nett cash of 5,322,519 pesetas (£212,900).

Fourteen railway concessions were granted during 1881, for a total aggregate length of 681 kilometres (425 miles). Their aggregate estimated cost amounts to 149,326,885 pesetas (£5,973,075), which gives an average estimate of 219,275 pesetas per kilometre (£14,054 per mile).

E. M.

The Hudson-River Tunnel. By W. S. SMITH.

(Transactions of the American Society of Civil Engineers, 1882, p. 314.)

The Hudson River, at the point at which the tunnel under it is now in course of construction, is one mile wide from bulkhead wall to bulkhead wall. The bottom consists of silt, under which, at the New York side, there is a bed of coarse sand, 20 feet thick, lying on coarse gravel, through which the tunnel is partly executed; but the tunnel is otherwise excavated in the silt, which has a minimum depth of 28 feet, and lies on rock. There is a maximum depth of 90 feet of water at low tides. The silt has the consistency of glaziers' putty, and is compact and tenacious. It consists of about 60 per cent. of combined silica, with quartz, manganese, alumina, and other materials. Its resistance to displacement was proved experimentally to be at the rate of 5,580 lbs. per square foot.

The tunnel proper extends from Fifteenth Street, Jersey City, to Morton Street, New York, and is 5,400 feet in length from shaft to shaft. It descends from the banks, with gradients of from 1 in 50 to 1 in 33⅓, to the deepest part under the channel, which is a level 500 feet in length, at a depth of 109 feet.

After an unsuccessful attempt to start from the shaft at New Jersey, with a single tunnel large enough to carry two lines of way, it was decided to construct two single-track tunnels side by side. These tunnels are nearly circular in section, having a diameter of 16 feet horizontally, and 18 feet vertically. They consist of a wrought-iron casing of ¼-inch plate, lined with brick, 2 feet thick, except for the middle portion, under the greatest pressure, in which 2½ feet of brick will be constructed. According to the mode of construction, a circular heading is driven into the silt, 5½ feet in diameter, eccentrically to the tunnel, about 2 feet off the centre. In the formation of the heading, a tube consisting of ¼-inch wrought-iron plates, called a pilot, is first carried forward, the circle being completed plate by plate, for the insertion of each of which a cut is excavated in the silt. The plates are bolted together. Another ring is next put in, in advance of the first ring, and bolted to it; and the pilot is thus continuously advanced until it reaches for a distance of from 20 feet to 30 feet beyond the face of the heading. The shell-plates of the tunnel itself are then put in and bolted together; each plate being stayed from the pilot, as a fulcrum, by radial bracing. The brick lining is inserted in 10-feet lengths, whilst the pilot and the shell may be carried forward uninterruptedly. The pilot acts as a rigid centre, and is manifestly of advantage in soft and varying material. When the material is firm and uniform, the pilot is not required.

A novel feature of the construction of the tunnel is the employment of compressed air, introduced by Mr. D. C. Hoskin, the projector and the constructor of the tunnel. An air-lock was first inserted in the wall of the shaft, projecting through it and into the outside material. Compressed air was forced into this lock, the door in its advanced end was opened, and the excavation of the material was commenced. As the excavation proceeded, the compressed air aided the plates and bracing employed to hold the materials in position and to keep out water. When the tunnel advanced some distance, air-tight bulkheads were built, consisting of 4 feet of brick and 12 inches of timber. Each bulkhead contained two air-locks, so adding to the convenience, and graduating the difference of pressure.

The south tunnel has been completed at the New Jersey side, for a length of 562 feet; and the north tunnel for a length of 1,000 feet. During the preparatory operations, the rate of progress averaged 53 feet of single tunnel per month for eight months. Subsequently, the rate of progress was augmented to 90 feet per month, and it is expected that it will reach 100 feet per month.

D. K. C.

Improvement of the River Broye.

(Bulletin de la Société Vaudoise des Ingénieurs et des Architectes, March and June, 1882, pp. 5 and 17.)

The plain of the Broye between Granges and Lake Morat is 13 miles long and nearly 2 miles wide at the broadest part. It is watered by the Broye, the Limbaz, the Arbogne, and the Petite Glane. The material brought down by the Broye has, in the course of ages, raised its bed above the level of the plain, and this fact, combined with the narrowness and sinuosity of its channel, and its slight fall, renders it quite unfit for discharging even slight floods, while the frequent occurrence of inundations prevents the proper cultivation of the plain.

Since 1826 various plans have been proposed for improving the river, and in 1853 a scheme was sanctioned for canalizing it, giving the bed a uniform slope of 0·00139, and removing a mill near the place where the river discharges into Lake Morat.

Several causes have rendered necessary the reconstruction of the earthworks on the lower length of the canal. Considerable erosion has taken place, the river at one point having formed a series of falls, which have deepened the bed some 10 feet more than was intended. The canal was made in a part of the plain in which the soil was loose, instead of in the old bed, which had been consolidated by deposits of gravel and sand. The depth has also been increased, owing to the lowering, by about 6 feet, of the level of the lakes of the Jura.

The new works are intended to provide for discharging the highest ordinary floods, but not those exceptional inundations which occur only at rare intervals. The Paper describes the method adopted for determining the discharging capacity of the new channel, and gives particulars of the flood discharge compared with the drainage area of several rivers. Other things being equal, the larger the drainage area, the smaller will be the discharge per unit of area. Thus the basin of the Rhine above the Bridge of Tardis is (without reckoning the glaciers) 4,230 square kilometres. The discharge at the bridge in 1868 was from 2,800 to 3,500 cubic metres per second, or, taking the larger figure, 0·83 cubic metre per second per square kilometre. The Aar, with a basin of 640 square kilometres, discharged 0·9 cubic metre per second. The Thour, with a basin of 1,724 square kilometres, discharges 0·8 cubic metre. The Thoess, with a basin of 390 square kilometres, discharges 1 cubic metre. The curve representing the discharge as a function of the area of the basin is of the form of an equilateral hyperbola, the asymptotes being the co-ordinates. The discharge of the Broye, with a basin of 347 square kilometres, was found from this curve to be 1·15 cubic metre per square kilometre per second, or a total discharge of 399 cubic metres per second. Mr. Ganguillet, from data obtained in the Thoess, gives the following formula:—$y = \frac{25}{5 + \sqrt{x}}$, in which y = the quantity of water discharged per square kilometre of the basin per second in cubic metres, x = the surface of the basin in square kilometres. For the Broye, $x = 347$, whence $y = 1 \cdot 10$, and the total discharge $= 347 \times 1 \cdot 10 = 382$ cubic metres. By means of gaugings in times of flood, 400 cubic metres had already been determined on as the discharge to be provided for in the new channel.

The transverse section of the new channel is to have a base of from 33 to 50 feet in width; the sides have a slope of 1½ to 1, for a height of 6 feet 6 inches, above which the slope is about 3½ to 1. The lower slopes are protected by pitching or paving, strengthened at the junction with the base by rockwork. The pitching is to be carried up to the height of ordinary floods. In some parts the protection is to be formed of continuous lengths of basket-work, in cylinders from 2 feet 6 inches to 3 feet 6 inches in diameter, filled with stones. These cylinders are laid along the foot of the slope in groups of three, two side by side, in a trench cut for their reception, the third placed over them. As the river washes away the earth from the trench, the cylinders gradually slide down, and form a lining to the slope. This system is known as Gumppenberg's, and has been successfully employed in Bavaria, and also by the Bernese engineers.

W. H. T.

The Condition of the Tidal Loire. By — Bouquet de la Grye.

(Comptes rendus de l'Académie des Sciences, Paris, vol. xcv., 1882, p. 506.)

A hydrographical survey of the tidal Loire was undertaken in 1881, with the object of ascertaining its navigable condition, of deducing its future changes from a comparison of its past and present states, and of discovering a remedy for its existing imperfect condition. Two new passes, unknown to the pilots, were found to have been formed near the outer bar, offering better channels of access to St. Nazaire. There is a yearly deposit in the river, between Nantes and St. Nazaire, of 770,000 cubic yards of sand and silt from the denudation of the mountain slopes of Auvergne and Forez; and the size of the channels in the estuary has diminished, in the last sixty years, at the rate of 73,000 cubic yards in a year. Since 1821 the average discharge per second of the tidal Loire at St. Nazaire has diminished by 2,320 cubic yards. Though a sort of equilibrium exists, below St. Nazaire, between the detritus brought down and the silt driven out to sea by the waves, the outer bar has been raised 2¼ feet since 1864, and probably in a few years will become a source of danger to large vessels coming to St. Nazaire. The methods of improvement proposed, which would slowly yet surely restore the river to its former condition, are: replanting trees; growing grass on the slopes; giving the Allier a proper course; and driving rapidly to the sea the 52,000,000 cubic yards of deposit, which have accumulated in the last sixty years, by an economical process indicated by the Author in his detailed account. Lastly, it is recommended that the improvement of the whole river should be placed under a single authority, since at present the engineers in charge of the tidal section have little control over the upper river where the injuries to the tidal channel really originate.

L. V. H.

Mississippi-River-Commission Reports.

(Report of the Chief of Engineers, U.S. Army, 1881, part iii., p. 2719, 2 plates, 1 woodcut.)

This Commission was appointed to complete the surveys of the Mississippi from the head of the Passes to its headwaters; to devise plans for the improvement of the river for navigation, and for the prevention of destructive floods; and to report on the respective merits of the jetty, levee, and outlet systems. The outlet system, by which a portion of the discharge of a river is allowed to escape through a lateral outlet into a secondary channel, is condemned as tending to produce shoals in the river below the outlet, owing to the reduction of the velocity of the current. Levees, or embankments, have invariably been erected on the banks of the Mississippi for the special purpose of protecting the alluvial lands from overflow, and therefore merely with reference to the high-water stage, and not in any way with the object of the improvement of navigation. The tendency of levees is to deepen and enlarge the bed of the river in flood time; and it is believed that from 1850 to 1858, when the levees were most efficient, the navigable condition of the river was better than it has been since the formation of numerous breaches in the levees between Cairo and Red River. The levee-system is not essential for securing a deep navigation channel; but the repair and maintenance of the extensive existing lines of embankments would hasten the improvement of the channel by the scour they cause in flood time. Moreover, the system of levees is absolutely necessary as a protection against destructive floods; it facilitates the maintenance of postal communication along the banks, and, by increasing the security of the adjacent lands, it promotes the development of trade. The repair of the breaches in the levees between Cairo and New Orleans, requiring 8,065,700 cubic yards of earthwork, is estimated at about £415,000. The defects in the navigable condition of the Mississippi are caused by the erosion of the concave banks, producing excessive widths, which occasion bars and shoals. In the case of all silt-bearing rivers flowing through alluvial deposits, like the Mississippi, the channel is most uniform and permanent where the width between the banks is uniform. Uniformity of flow follows from uniformity of section, and consequently there is an absence of eddies tending to erode the banks, and no liability to the formation of shoals. The regulation of the channel would also diminish the friction of flow, and by thus promoting the discharge lower the flood-level. The navigable state of the river below Cairo is good wherever the low-water width is not greater than 3,000 feet; but it is bad wherever this width is exceeded. It is proposed, accordingly, to regulate this portion of the river to a uniform width of about 3,000 feet at low-water; and, by suitable works, to cause the material scoured from the shallow places to deposit on the portions of the bed beyond the new low-water channel, and thus render the high-water channel more uniform. For these works it is proposed to employ mainly the usual constructions of hurdles, mattresses, brushwork, &c., which, being somewhat permeable, change the course of the current and favour deposit without violently arresting the flow, and which have been used with success at other places on the Mississippi, and on the Missouri. It is expected that these works will secure a depth of at least 10 feet at extreme low water over all the bars below Cairo. This plan of improvement has been termed the jetty-system. Reference is made to the state of the Upper Mississippi, and to the works of improvement in progress on it which have been previously described.[1] The navigable depth for a considerable portion of the year does not exceed 5 feet. Rocky beds and rapids obstruct the navigation at low-water at Rock Island and Keokuk. A channel 4 feet deep and 200 feet wide at extreme low water has been cut at Rock Island; and a canal has been constructed at Keokuk affording a minimum draught of 5 feet. Economy in carriage depends on the capacity of the barge, which increases rapidly with the amount of draught. Thus barges in the upper river, limited to 5 feet draught, carry about 12,000 bushels of grain, whilst those below Cairo, where 9 feet draught is often attainable, carry 20,000 bushels in 5 feet, 50,000 in 7 feet, and 60,000 in 8 or 9 feet. The Commissioners consider that it would be hopeless to anticipate any appreciable mitigation of floods from the employment of the system of reservoirs; but they think that reservoirs cheaply constructed might be useful in supplementing the low-water discharge of the improved river, and, by rendering a less contracted channel sufficient, reduce the cost of the training-works.

L. V. H.

Improvement of the Mississippi between the Illinois and Ohio Rivers.

(Report of the Chief of Engineers, U.S. Army, 1881, part ii., p. 1519, 17 plates.)

The dam across Cahokia Chute[2] has settled about 3 feet, but in other respects it remains in good condition. It has completely stopped the erosion of the Illinois shore, and has produced a deepening in the navigation channel west of Arsenal Island; and as this chute, or channel, has been considerably silted up by a recent flood, so as to stop the passage of water except when the river is high, a still further improvement is sure to follow. The training of the river at Horsetail bar has been continued. The concentration of the channel, the formation of new banks, and the reclamation of land, is accomplished by longitudinal and transverse hurdles as shown in the plans. The hurdles consist of brush wattled to piles placed 5 or 6 feet apart, or of mattress curtains fastened to rows of piles braced together. During the year 9,500 feet of longitudinal hurdles have been fixed, and twenty-two thousand transverse hurdles. These hurdles, including engineering and contingencies, cost respectively 16*s.* 3*d.*, and 14*s.* 8*d.* per lineal foot. Breaches are sometimes made in the new banks, but they have been promptly repaired. The expenditure at Horsetail bar in the year amounted to £23,850. Details are given in the report of works of protection and surveys at other places along this section of the Mississippi.

L. V. H.

---

[1] Minutes of Proceedings Inst. C.E., vol. lxv., p. 403.

[2] *Ibid.*, p. 401.

Inspection of the South Pass of the Mississippi.

(Report of the Chief of Engineers, U.S. Army, 1881, part ii., p. 1244, 11 plates, 3 woodcuts.)

Careful soundings are taken every year, both between and beyond the jetties, to ascertain what changes are taking place in the channel. The present report gives a detailed account of the changes which have occurred during the year 1880–1, and also of the works which have been carried out for the maintenance of the jetties and for the improvement of the channel through the Pass. The channel between the jetties was 30 feet deep throughout the year for a minimum width of 26 feet, and the 26-feet channel had a minimum width of 200 feet. Beyond the jetties, the 30-feet channel had a minimum width of 60 feet, and the 26-feet channel a minimum width of 200 feet. Through the Pass, above the jetties, a 26-feet channel was maintained during the year with a minimum width of 160 feet; and at the date of the Report, July 1881, there was a channel, from the main river into the gulf, whose least depth is 27 feet, and least width 160 feet, and a steamer drawing 25 feet passed through. The bed of the Pass has been scoured in some places and raised in others, but the amount of deposit has exceeded the scour by only about one-tenth of a foot in the year. During the previous five years the total depth of deposit amounted, on the average, to 2½ feet. Wing-dams were built, projecting into the Pass in places where shoaling had occurred, so as to concentrate the current and promote scour. The east jetty has been consolidated by sinking cribwork at its seaward end, by building a rubble wall on the top of the concrete blocks, and by other minor works. The most interesting part of the report relates to the changes which have taken place in the gulf beyond the jetties, as indicated by the elaborate survey of the bottom, extending from the ends of the jetties, over a fan-shaped area, out to a depth of 100 feet. This area, over which soundings have been taken regularly since 1876, is 2 square miles in extent, and 1¼ square mile of this area has been subdivided into twenty-one divisions, of which the mean depths are obtained and compared with former years. It appears from this comparison that whilst no appreciable change in the mean depth, over the whole area, took place in 1879–80, there has been an average deposit of 2 feet 4½ inches over this area in 1880–1; whilst between 1876 and 1881 only five divisions have become deeper, the greatest average deepening being 1½ foot, and the remainder have been raised to a much greater extent, in one case as much as 12½ feet. The divisions where scour has occurred all lie near the end or in front of the jetty-channel, whilst shoaling has taken place in all the outer divisions, both in front and at the sides. More time must elapse, however, before any definite conclusions can be drawn from these indications, as they differ somewhat from what a comparison between the states in 1876 and 1880 would have indicated. The changes which are taking place in this part of the gulf are also shown by a comparison of the positions of each 10-feet line of depth from 20 to 100 feet. These lines, with the exception of the central and eastern portions of the 20- and 30-feet lines, and the central portion of the 50-feet line, have all advanced, the eastern portion showing the greatest advance, which increases with the depth, whilst on the western side the advance decreases with the depth. It is evident from the diagrams that the area directly in front of the jetties is undergoing the same changes as the rest, though smaller in degree, and that silting up is taking place in the gulf in front of the jetties; but it is as yet too early to determine the precise course the changes will take, and how soon, and to what extent, the shoaling will affect the outer channel.

L. V. H.

Protection of the Navigable Waters of California from Injury from the débris of Mines.

(Report of the Chief of Engineers, U.S. Army, 1881, part iii., p. 2485, 4 plates.)

The process of the extraction of gold from the auriferous gravel of California causes large quantities of detritus to be washed into the rivers. During the last few years hills have been disintegrated by machinery for the gold they contain; and this process, known as hydraulic mining, has led to a large accumulation of débris in the river-beds. The deposits vary in size from large stones to fine sand and clay. The débris is gradually brought down from the steep upper rivers, and is strewn in flood time over the flat plains below. The river-bed is thus obliterated through the plains, and the alluvial lands bordering the river are converted into a waste of sand and gravel, through which the river winds in a changeable channel in its low stage, and in its high stage floods a large area. Seven tributaries of the Sacramento are estimated to bring down 38,000,000 cubic yards of material in a year. To prevent the further devastation of valuable alluvial lands, dams of brush were placed across the Yuba and Bear rivers in 1880, at places where the river, spreading out above, affords a considerable space for the accumulation of deposit. The dams have been raised to an average height of 7 feet and 6 feet respectively, and have been made broad enough at the base to admit of a considerable raising of the dam. The reservoir space above the Yuba dam will impound 42,000,000 cubic yards, and above the Bear dam about 10,000,000 cubic yards of material. The dams are formed of the trees and brushwood growing on the spot. The Yuba dam is 8,700 feet long, and its height varies from 4·7 to 11·7 feet; the Bear dam, having a similar height, is 5,900 feet long. The only effect of the floods of 1880–81 was to produce a slight settlement of the dams; and the dams will be strengthened and protected from decay by becoming embedded in detritus. It is hoped that this system of dams across the upper rivers will check the devastation of the alluvial lands by restraining the mining débris from reaching the plains.

L. V. H.

Underground Dams in the Döllinger Coal-Mines.

By A. Siegmund.

(Zeitschrift des österreichischen Ingenieur- und Architekten-Vereins, xxxiv. p. 69.)

On the 10th of February, 1879, a sudden irruption of water took place in the Döllinger coal-district at Osseg in Bohemia, which resulted in the loss of twenty-three lives and the stoppage of several adjacent mines, throwing nearly a thousand hands out of work. Three days later a further consequence of the accident became apparent in the sinking of the water-level in the thermal springs of Teplitz, about four miles distant, followed shortly afterwards by the entire cessation of the flow, and for a time destroying the prosperity of one of the most favourite balneological stations in Europe. In order to bring back the mines to a working condition, it became necessary to execute the works which are described by the Author. In the first place the proprietors of the five mines interested erected a pumping-engine of 360 HP., with four lifts of 30-inch pumps capable of lifting 24 cubic metres (5,280 gallons) of water through a height of 205½ feet. In four months' time the lower works were freed from water, the adjoining collieries resumed work, and the locality of the accident could be visited. This was in the end of a cross cut in the third or lowest level of the Döllinger mine, about 197 feet below the surface, and in the immediate vicinity of an open fault called the *Russkluft*, which throws the seam about 16 feet down to the north. The seam of brown coal is about 50 feet thick, which is only of clean workable quality in the middle, the upper part containing much pyrites, and the lower alternations of shale. When first visited by the Author, in June 1881, water was pouring in at the rate of 12 cubic metres per minute, forming a cascade 5 feet broad and 6½ feet fall, the temperature being 17° Reaumur (65° Fahrenheit); and the fault was washed out, leaving an open space of 210 cubic metres, extending from the third or lowest level to the second above it.

The works for shutting out the water and securing the levels consist of spherical segmental dams in brickwork at the points of irruption, and in the main levels on either side of the pumping-engine shafts, a complete circular or elliptical walling of the bottom level for about 200 yards, and brickwork stoppings and fillings at the crossing of the main fault.

In order to get at the principal fracture, a branch level was driven so as to turn the flow of water, the ground being carefully sounded in advance by boring, and a partial dam built to regulate the current when tapped. For some time the water flowed in both directions, but at length a sudden fall of coal blocked the original fracture, and the current was entirely turned through the new channel.

The first operation, after securing the ground with timber, was to build a provisional dam of masonry and cement, after which the ground was cut out to the shape of the spherical dam. This is a segment of a sphere of 9 metres radius, with the larger diameter, 3·47 metres wide, outwards; the smaller one is 2,610 metres, and the length measures radially 3·75 metres. It is traversed by a cast-iron pipe of 600 millimetres (2 feet) bore, closed on the side of the level by a sluice-valve. The dam is made of sound well-burnt bricks, not glazed or clinkered, set in a mortar of one-third cement and two-thirds sharp sand, built in annular shells of 600 millimetres thick. It was originally intended to make the dam a segment of 40°, but from the doubtful character of the ground, and the vicinity of the dangerous open fault, the smaller arc of 25° was used in order to be more certain of the foundations. The dams in the lower level are somewhat similar to the preceding, but double, being made up of two spherical segments of 40° central angle, about 3 metres apart, which space is filled with a cylindrical well. A cast-iron pipe with sluice-valve, that described above, is built into both. The walling of the deep level is partly elliptical, 1·3 metre by 1·8 metre, and partly cylindrical of 1·3 metre diameter. The most difficult part of the work was the cleaning out and securing of the fault, a principal obstacle being defective ventilation, as, although a fan was kept continually at work, the prevailing temperature was as high as 37° Reaumur (115° Fahrenheit), so that it was necessary to work with double relays of men, who were relieved at intervals of fifteen minutes.

In all 1,667 cubic metres of masonry were executed, for which 442,000 bricks, 1,164 tubs of Portland cement of 0·114 cubic metre each, 447 cubic metres of sand, 15 tons of lime, and 1 cubic metre of squared stone were required. The pipes and valves amount to 18¾ tons of castings, and 576 tons of coal were required by the engines during the operations. The total cost, including that for the engines, was £7,892 6*s.* (at 10 gulden per £).

As soon as the mortar of the main dam was sufficiently hardened the channel on the branch level was gradually diminished, so that the water was turned through the pipe in the dam until the entire flow took this direction. The branch level was then completely blocked with brick and cement walling, and the water was allowed to flow for eight weeks, until the necessary stoppings and walling on the other parts of the level were finished. The valve was then screwed down, and the influx of water entirely ceased. This took place on the 20th of May, 1882, and eight days later the water-level shaft in the Stadtbad spring at Teplitz, which had up to that time been continually falling, began to rise, and is now

5 metres higher than at that date, although the draught upon the well is 12,000 cubic metres more than it was in the previous year, thus showing that the reservoir of the spring in the porphyry began to fill as soon as the influx of water into the mine was stopped.

H. B.

Current-Meter Measurements in the Rhine, below the Bridge of Constance. By ADAM BAUM.

(Allgemeine Bauzeitung, vol. xlvii., 1882, pp. 53, 80.[1])

In studies for lowering the high-water level of the Boden See, it became necessary to ascertain the discharge with different levels of the water in the lake. The measurements were made at a cross-section of the Rhine, below the bridge of Constance, near the point of discharge from the Boden See. For ordinary conditions of the water-level the stream has here a conveniently bounded profile. At the highest water-level the stream floods the banks, but the quantity flowing beyond the limits of the channel is vanishingly small compared with that flowing in the ordinary bed. The cross-section has a breadth of 136·13 metres (450 feet), and a maximum depth of 11·4 metres (37⅓ feet).

Surface-fall.—To determine this nine gauges were fixed, six on the left shore and three on the right. The highest on the left, the Rhein-thorthurm gauge, was that to which all measurements were referred. To determine the surface-fall with different levels of the water in the lake, thirty-three sets of readings of the levels on the gauges are available.

The Author has plotted these results, which exhibit great differences. A discussion of them leads to the formula—

$$J = 0 \cdot 000067541 - 0 \cdot 00000173474\, u,$$

where J is the relative fall, and u the height of the water-surface on the principal gauge. The zero point on this gauge is at the highest known water-level.

Measuring Apparatus.—The supports for the current-meter were placed on a platform between two coupled boats, each 33 feet long by 6 feet beam. The current-meter was attached to a fixed vertical T-iron (4 inches by 2⅜ inches). The arrangements for fixing this, and for raising and lowering the meter are described. The current-meter was fixed with its axis normal to the cross-section, and was not directed by a rudder or vane. It had a screw of 4·7 inches diameter driving a worm-wheel, making one rotation to one hundred of the screw. The worm-wheel carried a pin making electrical contact once in each revolution, so that a bell sounded at each hundred rotations of the screw.

---

[1] See also "Der Bodensee und die Tieferlegung seiner Hochwasserstande. Von Max Honsell." Stuttgart, 1879. This book is in the Library of the Inst. C.E.

Determination of the Constants of the Meter.—The observations give merely the time of one hundred revolutions, and the velocity of the water is not directly proportional to the speed of the meter. It is necessary, therefore, to find a relation between the time z per hundred revolutions, or the number of revolutions n per second, and the velocity v of the water. Then $n = \frac{100}{z}$, and $v = f(n)$ is the relation required. To determine this relation a number of trials were made in still water. The boats carrying the meter were guided by a fixed wire rope and moved by a windlass. One observer noted the time of one hundred revolutions, and another the distance traversed along the guide rope. Applying the method of least squares, the Author finds the equations—

(α) In the direction of the wind,

$$v = -0{\cdot}1271 + 0{\cdot}3706\,n.$$

(β) Against the wind,

$$v = 0{\cdot}12779 + 0{\cdot}25104\,n.$$

(γ) From both combined,

$$v = 0{\cdot}02514 + 0{\cdot}259533\,n,$$

v being in metres per second. The discrepancy of these equations, which is hardly explainable as due to the wind, which was always very slight, leads the Author to discard them. The negative value of one of the constants is also impossible. The proceeding ultimately adopted was this. The rotations per second and corresponding velocities were plotted in a diagram as abscisses and ordinates. Through the points so found a provisional straight line was drawn. One point on this line near its upper end was assumed as accurately fixed, and from this and the $m - 1$ other values of n and v equations were formed to determine the two constants. The arithmetical mean of these values gave the following equations:—

(α) In the direction of the wind,

$$v = 0{\cdot}06037 + 0{\cdot}28168\,n.$$

(β) Against the wind,

$$v = 0{\cdot}08844 + 0{\cdot}26892\,n.$$

(γ) Mean of both,

$$v = 0{\cdot}0744 + 0{\cdot}2753\,n.$$

The difference in the first constant is narrow, and indicates a small current reverse to the direction of the wind, at the depth at which the meter was placed. The Author discusses some results with the meter differently supported, and concludes that formula

(γ) may be used in reducing the observations with the meter in all cases.

Surface-velocity Curves.—The Author gives the surface-velocity curves for three conditions of the river. The curves are too irregular to be approximated to any geometrical figure.

Vertical-velocity Curves.—These in general are similar to those obtained in other researches. The exceptional form of some of the curves is due, (1), to the friction of the contact-maker which, when the velocity was very small, had a proportionately greater effect in retarding the meter; (2), to peculiarities of the river-bed, the irregularities of which influenced the position of the point of greatest velocity; (3), to the nearness of the point of discharge from the Boden See. The velocity at each point was due partly to the surface fall at the section, partly to the pressure of the Boden See considered as a reservoir. The peculiarly deep position of the point of greatest velocity in some of the curves may be explained as due to these causes.

Variations of the Velocity at one Point.—The meter was fixed in a selected position in the cross-section, and from the bell signal the time of each successive hundred revolutions was taken for a period of two hours. The results plotted show a continual variation of the velocity. Further, a wavy line can be drawn through the observations, taking a mean position between the shorter oscillations, and having a length from crest to crest corresponding to a period of about an hour.

In order, therefore, to ascertain the accurate velocity at any one place in the cross-section, it would be necessary to extend the observations over the whole of such a period. That is, to observe the time of about six thousand revolutions of the meter. But as this is not practicable, the observations taken in short periods must always be affected by considerable irregularities.

The Author then considers observations on a series of verticals, to determine the variation of velocity with varying level of the river. By dividing the area of a vertical velocity-curve by the depth of the river, the mean velocity at that vertical in the given condition of the river is obtained. Setting off these mean velocities as abscisses, with the gauge-reading as ordinate, curves are obtained giving the mean velocity at each vertical for every condition of the river. The curves show great irregularities. These are due to various causes. Partly to the variation of the velocity at each point already discussed, partly to the rising or falling of the water during the observations, partly to the boat not being fixed exactly normal to the cross-section, and partly to the whirling motion of the water, which the Author discusses at length.

The discussion of the vertical and horizontal velocity-curves cannot be rendered intelligible without the original drawings.

Discharge.—From the curves of the observations the Author obtains a Table giving the mean velocity on each vertical for each foot fall of the water-surface of the gauge from 2 feet to 12 feet. The discharge for each of these conditions of the river is obtained, by multiplying the area of the cross-section between each pair of verticals by the mean of the mean velocities on those verticals. For a portion of the section towards each shore the velocity for low conditions of the river was zero. The mean radius and mean velocity has also been reckoned for each of the same gauge-readings.

Comparison of measured Discharge with Formulas.—The Author has calculated the mean velocity of the river for each level from 2 feet to 12 feet on the gauge by seventeen well-known formulas. The calculated values show very great departures from the measured velocities, especially for low conditions of the river. The Author then recalculates from the measured velocities the constants of the formulas, choosing the observations with 5 feet gauge-reading for formulas with one constant, and those at 5 feet and 10 feet for formulas with two constants. Recalculating the mean velocity for all levels of the river with these new constants, there are still extremely wide departures from the measured velocities.

The Author then describes some observations with surface-floats in those parts of the river where the velocity was too small for the accurate use of the current-meter. Also some observations with rods made to check the observations with the current-meter. The rods were of wood, projecting 20 to 24 inches above water, and reaching to within 16 to 24 inches of the bottom. The agreement with the current-meter observations is satisfactory, but the floats give in general a slightly higher value of the mean velocity than the meter. The difference is due partly to the path of the rods not being quite exactly known, part to the rods not extending to the bottom of the river.

W. C. U.

Improvement of the Port of Bilbao.

(Revista de Obras publicas, 1882, pp. 172 *et seq.*)

Important works of improvement have been carried on with considerable activity in the River Bilbao since the end of 1878, and will probably be entirely completed in the course of the year 1885. These works are all situated in the portion of the river between the town of Bilbao and the bar (a distance of 9¼ miles), and may be divided into four sections. 1st. The works in progress between the town and the Lazaret. 2nd. The Elorrieta cutting, situated a little below the Lazaret, at the confluence of River Cadágua, and about 4½ miles above the bar. 3rd. The Mount Axpe cutting, situated about 2¾ miles above the bar. 4th. The works in progress in the neighbourhood of the bar itself.

1st. The works in progress between Bilbao and the Lazaret consist of different lengths of river wall and quays on both banks, in the neighbourhood of the town, and of some heavy dredging over the whole length of the section, the most important portion of the latter work being the removal of some gravel banks called "Los Churros," which have formed for centuries the most serious obstacle to navigation in this part of the river.

Up to the end of June 1882, about 437 yards of quays and walls had been completed. The dredging was still in a somewhat backward state. About £12,500 had been expended upon this length for work done, and £15,000 for the purchase of land required for wharves, &c.

2nd. The Elorrieta cutting.—By this cutting a new channel, on a gentle curve, has been substituted for a very sharp bend which formerly occurred in the river. The new channel is about 100 metres (328 feet) wide, and is enclosed between sloping masonry walls. The left bank wall is 710 yards long, the right bank one 939 yards. This important piece of work is almost entirely completed. The sum expended upon it up to June 30th was about £44,200.

3rd. The Mount Axpe works consist in a deviation of the river of considerable length; the construction of sloping river walls, similar to those of the Elorrieta cutting, on both sides of the new channel, and of a basin which is to be formed at the foot of Mount Axpe, in the portion of the old channel cut off by the deviation.

The river at this place presented formerly serious obstacles to navigation, caused by the sharp bend occurring near the confluence of River Galindo, and by the existence in mid-channel of a sunken rock called "El Fraile." This rock now forms part of the right river wall, and a new channel has been cut and dredged through a portion of the left bank of the river. This new channel curves to a radius of 800 metres, and has a minimum navigable width at present of 80 metres, which will be further increased by dredging.

This portion of the works is not yet finished, but even in its present incomplete condition has served already to produce a powerful scour, which is calculated to have removed from its neighbourhood, in a few months, over 40,000 cubic yards of sand.

4th. The works in the neighbourhood of the bar consist of a pier 874 yards long, which is to form a continuation of the old Portugalete mole on the left bank of the river. The new pier begins about 546 yards above the present site of the bar, and will end about 328 yards beyond it, in a depth of 20 feet below the level of low-water spring tides.

This pier is composed: 1st, of a light iron structure, built upon bays of two screw piles, 20 feet apart longitudinally; 2nd, a foundation of rip-rap, tipped either from the iron pier or from barges, extending a little way on either side of the iron pier, and reaching to the level of low-water spring tides. This foundation will be protected from scour on the river side by concrete blocks; 3rd, a solid concrete wall, 13 feet 1 inch high, built upon the rip-rap between the screw piles, and reaching up to the level of ordinary high tide. The top of the iron structure is 11 feet 6 inches above the top of the wall, and consequently 24 feet 7 inches above the low-water spring-tide line.

At the end of June 1882, sixty-nine spans of iron pier had been completed, measuring in all 1,359 feet in length. The rip-rap had been placed over a length of 382 yards, and 98 yards of concrete wall had been built.

The sums spent upon the Mount Axpe section, and upon the pier at the bar, amounted to about £31,000. All these works, though as yet far from completion, have had already a marked effect in improving the navigation of the river, and in lowering the bar. The following are the results so far observed, and solely due to increased scour at the bar.

1st. On the line of deepest soundings, the minimum depth below low spring-tide level has increased from 3 feet 9 inches to 5 feet 3 inches.

2nd. The minimum width of channel enclosed between the lines of 3 feet 3 inches soundings, was formerly 246 feet, and is now 524 feet.

3rd. The length of the bar has been considerably diminished. The seaward edge has remained almost stationary, whilst on the landward edge the line of 9 feet 10 inches soundings has travelled seaward 197 feet, and the line of 6 feet 6 inch soundings has moved in the same direction 131 feet.

These effects are expected to increase enormously as the concrete wall progresses. The rip-rap and the iron pier, which so far form the main portion of the work executed, have of course comparatively limited action in directing the scour.

The amount of dredging executed in the three lower sections (principally in the new channel at Mount Axpe) was 221,000 cubic yards, and cost £7,400.

The total amount expended on the whole of the works up to June 30, 1882, has been about £110,100.

The total estimate of the Bilbao River Improvement is £407,400.

E. M.

Improvement Works at Calais Harbour.[1]

By H. de la Brosse.

(Annales des Travaux publics, September 1882, p. 699, 2 pl.)

The existing harbour of Calais consists of a dock of about 5 acres, with quay-walls founded at the level of low-water spring tides, and of a tidal or outer harbour, having rather over a mile of quay walls founded at the level of low-water neap tides. A passage with a single pair of gates, 55¾ feet wide, connects the dock and outer harbour. A sluicing basin of 141 acres serves to maintain the depth of the jetty-channel.[2] The Calais canal, which communicates by a lock with the tidal harbour, connects the port

---

[1] Minutes of Proceedings Inst. C.E., vol. lxx., pp. 4 and 75.

[2] *Ibid.*, vol. lxx., Plate 1, Fig. 2.

with the inland waterways. In spite of the inadequate accommodation, the total tonnage of vessels entering and leaving the port, which in 1875 was about 700,000 tons, had risen in 1880 to 1,500,000 tons. New works were commenced in 1876, on which an expenditure of £600,000 has been authorised. These works consist of a dock of 25 acres, with quay-walls founded 8¼ feet below low-water spring tides, in front of which there will be a depth of about 24½ feet at neap tides; a tidal basin of 15 acres, with a quay-wall founded 13 feet below low-water spring tides, set apart for the mail packet service; and, lastly, a sluicing basin, of nearly 250 acres, opening into the middle of the jetty-channel.[1] The dock will communicate with the tidal basin by two locks, side by side, one 438 feet long and 69 feet wide, and the other 451 feet long and 46 feet wide. The entrance-channel beyond the jetties is to be deepened to 11½ feet below low-water spring tides; the eastern jetty is to be rebuilt, and the west jetty lengthened; a graving-dock is to be made, hydraulic machinery introduced, and other minor works to be executed. The entrance for the dock has been completed, and the excavation of the sluicing-basin is in progress. The outer dam of the sluicing-basin is formed of an embankment of sand, protected on the seaward side by pitching, with piling at the toe of the slope. The driving of these piles into the fine wave-beaten compact sand forming the beach was extremely difficult, till the method of facilitating it by the injection of water at the foot of the piles was devised, with excellent results.[2] This plan of driving, indeed, proved so satisfactory that it has been used, with improvements, for nearly all the pile-driving on the works. The lock-foundations consist of a concrete floor, founded at a uniform level, but varying in thickness from 5 to 6½ feet; the concrete is, however, carried down deeper at the ends and under the gate-floors, to prevent the flow of water so liable to occur in wet sandy soils. The locks have each four pairs of gates; one pair of ebb-gates at each end of the lock-chamber, one pair of flood-gates at the seaward end, and one intermediate pair of ebb-gates dividing the lock-chamber unevenly, so as to form one small chamber and one moderate-sized chamber, thus adapting the lock to various sizes of vessels. There are also to be two swing-bridges over each lock at either end, beyond the lock-chamber, so that the traffic need never be interrupted. The excavation of the sluicing-basin is being carried out, like the excavation for the dock was previously, partly by manual labour and partly by an excavator with a chain of buckets.[3] One minute suffices to fill a wagon holding 8 cubic yards, and a train of thirteen wagons, being loaded within thirteen minutes, the rate of excavation would be exceedingly rapid if the distance the material has to be conveyed did not cause long delays. Nevertheless the amount of earthwork

---

[1] Minutes of Proceedings Inst. C.E., vol. lxx., Plate 1, Fig. 2.

[2] *Ibid.*, vol. liii., p. 312.

[3] *Ibid.*, vol. lv., p. 378.

excavated by the excavator and removed in a day has often exceeded 2,600 cubic yards, and in a summer day of fourteen hours, as much as 3,140 cubic yards have been moved. The sluicing-basin is to be excavated to 8¼ feet below high-water spring tides. It has been decided to provide five sluice-ways, each 19¾ feet wide, which, affording together a clear opening of 92 feet, will enable a depth of 5 feet of water over the area of the basin to be discharged in forty-three minutes, the period during which the sluicing current is really efficient. Each sluice will be provided with a pair of flood-gates, to keep out the tide when sluicing is not going on, and with a single wrought-iron sluice-gate turning on a nearly central pivot. The piers between the sluice-ways are to be 11½ feet wide and 69 feet long. The aprons of the sluices have been made of a considerable length, both above and below the gates, to protect the sluices from being undermined, to which the rush of water and the sandy soil renders them peculiarly liable. Two trenches of concrete, 6½ feet thick, have been carried down 16½ feet below the sill, across the ends of the sluice-ways, with the same object. The floor of the sluices consists of a bed of masonry, 5¾ feet thick, resting upon a bed of concrete of the same thickness. The dock walls, which have been commenced, rest upon a concrete foundation between two rows of sheet piling, and are built of rubble masonry set in cement mortar. The face of the wall has a straight batter of 1 in 10. A few particulars are given relative to the dredging operations at the entrance,[1] and details are given of the manner in which the driving of piles and planks was accomplished by the injection of water at their feet. At the locks, where all the piling was within a small area at a short distance from the pumping-station, some of the water from the excavations was pumped up from the pipes by an auxiliary pump into a reservoir placed about 50 feet above the level of the piling, and this water, being led into a long iron pipe 8 inches in diameter, could be drawn off through taps placed at intervals along the pipe. This method of supplying water under pressure was used in driving all the piles and planking for the lock foundations, the total length of sheeting being about 2,000 feet. For the dock-wall foundations the distance was too great to convey the water from the pumps, so a little Tangye steam-pump was placed on each pile-engine to inject the water. This plan gave excellent results, and is being now used for the piling at the sluices. A detailed account is given of the actual cost of the sheeting for the dock-wall foundations, the total expenditure amounting to £6,170; and comparing this with a detailed estimate of the cost of the same work if it had been done without the aid of the water-jet, it appears that the saving effected by the injection of water amounts to £11,520, or nearly double the actual cost of the work.

---

[1] A fuller account is given by Messrs. Plocq and Guillain in the discussion on "Harbours and Estuaries," Minutes of Proceedings Inst. C.E., vol. lxx., p. 78.—L. V. H.

Since the commencement of the improvements in 1876 up to the end of 1881, the total cost of the works has amounted to £460,000.

L. V. H.

Improvement of Chicago Harbour.[1]

(Report of the Chief of Engineers, U.S. Army, 1881, part iii., p. 2151, 1 plate, 2 woodcuts.)

The cribwork breakwaters forming the new harbour or basin at the mouth of the Chicago river, of which a description has been previously given,[1] are now completed. Three cribs, each 100 feet long, have been sunk for the southern breakwater since the last report, bringing it up to its full length, and the superstructure was then built upon the top, the whole work having been completed by the end of August 1880. The total length of the southern breakwater is 3,000 feet, and it has a width of 16 feet, except for a length of 300 feet which is 30 feet wide. The cribs rest upon piles cut off to a level of 14½ feet below the surface of the water. This is the first completed crib breakwater built on such a foundation, and it has proved perfectly satisfactory. Dredging is being carried on for increasing the depth inside the harbour, and 152,470 cubic yards were excavated in the year 1880–1. An exterior detached breakwater 5,436 feet long, situated about a mile to the north-east of the harbour, has been designed for the purpose of protecting the entrance, and forming a refuge-harbour outside the harbour just completed. Cribs 100 feet long, 30 feet wide, and 22 feet high, had been built for this breakwater at the date of the report, and were to be put in place on the first favourable opportunity. The breakwater was to be commenced in the centre, and extended simultaneously both ways. As driving piles in the open lake in a depth of from 25 to 32 feet of water, and cutting them off to an exact level, would have proved an expensive foundation for the cribs, it was decided to adopt the expedient of doing away with the side walls of the cribs, at a depth of 20 feet, and filling up the interval between the bottom of the lake and the commencement of the side walls with rubble stone. This plan, it is anticipated, will afford increased stability and prevent unequal settlement of the cribs.

L. V. H.

Improvement of the Harbour of Calumet, Illinois.

(Report of the Chief of Engineers, U.S. Army, 1881, part iii., p. 2168.)

This harbour is being improved in a manner of which there are numerous instances contained in the appendices to the report, and of which this work may serve as a typical example. The method

---

[1] Minutes of Proceedings Inst. C.E., vol. lxiv., p. 382.

consists of building out parallel piers of cribwork from the shore, on each side of the harbour entrance, into deep water in the lake, and dredging the channel between the piers to the required depth. The piers for improving the entrance to the Calumet river were commenced in 1870; they are placed 300 feet apart, and by the end of June 1881 the length of the northern pier was 3,190 feet, and of the southern pier 1,510 feet, and their extremities more than 1,356 feet and 1,250 feet respectively in advance of the existing shore line. The piers are formed of cribs resting upon foundation piles. Vessels can now enter drawing 13 feet at low water, whereas formerly the depth over the bar was only from 4 to 7 feet, and the channel winding. The amount of dredging already accomplished was 298,500 cubic yards; and the total expenditure on the works up to the end of June 1881 was £61,000. It is proposed to extend the north pier 500 feet, so as to reach a depth of 20 feet, and it is estimated that about 85,000 cubic yards of dredging would be required to deepen the channel, for a sufficient width, to a depth of 16 feet. It is estimated that these additional works will raise the total cost of the improvement to £74,400. The entrance-channel will have to be maintained by periodical extensions of the north pier. The rate of this extension will depend on the rate of advance of the shore line to the north of the pier, which amounted between 1870 and 1880 to 1,207 feet, or an average annual advance of 120·7 feet; but in 1880–1 the advance was only 21 feet. The harbour is of considerable importance owing to its forming the approach to the port of South Chicago, whose commerce is rapidly increasing.

L. V. H.

On Breakwaters, with reference to the System adopted at Boulogne Harbour.

(Annales des Travaux publics, September 1882, p. 705, 11 woodcuts.)

Before the engineers entrusted with the design of the new harbour at Boulogne decided on the type of breakwater to be adopted, they investigated the results attained by the works of a similar kind already constructed, and this article gives a summary of the conclusions they arrived at.

Jetties may be divided into two principal classes, open and solid. Open jetties are used where it is desirable not to interfere with littoral currents, nor to arrest the travel of shingle or sand, and also for the sake of economy; but they do not afford good shelter from waves. The jetties at the entrances of French harbours, and at the mouth of the Adour, are cited as instances of this system of construction.[1] Floating breakwaters, which belong to this class, and have been tried at La Ciotat and Brighton, have proved unable

---

[1] Minutes of Proceedings Inst. C.E., vol. lxx., pp. 4, 6, and 20, Pl. 1 and 3.

to resist violent storms. There are three kinds of solid jetties, or breakwaters; the rubble-mound system with long slopes; the vertical wall system; and the mixed system of rubble mound surmounted by a masonry superstructure. The long slope breakwater has been occasionally constructed of fascines, as at the Hoek-van-Holland;[1] but this method has been sometimes found wanting in durability. This system of breakwater is generally formed of a mound of rubble stone, of which Cherbourg, Plymouth, Kingstown, and Delaware breakwaters are given as instances. Experience, however, has proved that this type is not permanent, and is especially liable to be displaced by the sea above low-water level. This has led to the protection of this portion of the seaward slope of the mound, by pitching with large stones set in cement at Plymouth, by large concrete blocks at Cherbourg, and by large blocks of stone at Delaware. The breakwater at Algiers is composed entirely of a mound of large concrete blocks, made at first 13 cubic yards and subsequently nearly 20 cubic yards each. Its slope stands at an inclination of 1¼ to 1; whereas the portion of the outer slope of a rubble stone breakwater exposed to the action of the waves, extending to a depth of about 16 feet below low water, assumes a slope of between 5 and 10 to 1 according to the site. In constructing the Marseilles breakwater the best points of both the Cherbourg and Algiers breakwaters were adopted. Thus, as at Cherbourg, the lowest portion of the mound has been composed of all the material from the quarry, the smaller stones, however, being placed at the bottom and surrounded at the sides and above by the larger stones; and large concrete blocks, like those at Algiers, have been placed on the most exposed portion of the seaward slope. One portion has been in existence twenty years, the rest was completed six years ago, and the whole remains in good condition.

There are some objections to the vertical-wall system, of which Dover breakwater is a typical example. The foundations are costly to prepare if the bottom is rock, and are liable to be undermined or to settle in sand. The construction of the wall is slow and costly. At Dover, the progress was not more than from 100 to 115 feet in a year, and its cost exceeded £920 per lineal yard; whereas the breakwater at Algiers cost only £585, and the Joliette breakwater at Marseilles as little as £200 per lineal yard. Moreover, as the blocks below low water cannot be cemented together, fissures are liable to form under the action of the sea, or from settlement, which are difficult to repair, and may widen rapidly into breaches. Messrs. Stoecklin and Laroche consider that both theory and practice show the vulnerable part of the vertical wall to be below low water; whereas, on the contrary, the vulnerable part of the rubble mound is above low water. Selecting the most stable portion of each system, they were led to adopt the mixed system, of a mound below, and a wall above low water, for the Boulogne break-

---

[1] Minutes of Proceedings Inst. C.E., vol. lxx., Pl. 3, Figs. 6 and 7.

waters.[1] This system, however, has also weak points: the mound requires time to consolidate before building the superstructure, and even then it settles under the weight of the superstructure, causing the latter to bear unevenly on the mound, or even to crack, as happened at the Socoa breakwater of St. Jean-de-Luz. The engineers have endeavoured to provide against these defects at Boulogne by giving a wide surface at the top of the mound, and protecting the portion in front of the wall with large stones, or bags of concrete on Mr. Dyce Cay's system.[2] The northern breakwater is to consist merely of a rubble mound, carried up to the level of low-water neap tides, without any superstructure. Curved faces to breakwater walls, for the purpose of increasing the base and reducing the shock of the waves, are condemned, from the experience of injuries suffered at the Socoa breakwater. Thus the toe of the wall is liable to be cracked by unequal settlement, and to be broken by the recoil of the waves which is intensified by the curved form leading the waves up. At St. Jean-de-Luz, the waves sometimes rose 100 feet above the wall, and, driven inwards by the wind, fell with great force on the top of the wall, injuring the quay and rendering it unapproachable. Projections on the face are also objected to, having frequently been the cause of injury to the St. Jean-de-Luz quay, and having led in 1877 to the destruction of 1,000 feet of the parapet of the Dover pier. Accordingly the face of the superstructure of the Boulogne breakwater has been designed perfectly straight, with only a slight batter, and without any projections.

L. V. H.

Coal-Boxes for Stationary Boilers. By H. Walther-Meunier.

(Bulletin de la Société Industrielle de Mulhouse, 1882, p. 363.)

Messrs. Schaeffer, Lalance, and Co., after they had got a siding from the railway into their works, were under the necessity of keeping the coal for their steam-boilers near the entrance, to provide against the irregularities of supply, and of maintaining a permanent staff for this purpose and for transporting the coal to the boiler-sheds. Breakage of coal, too, was caused by the frequent handling.

To obviate these disadvantages, a row of coal-boxes, or covered sheds, has been constructed, facing a row of eight boilers, one facing each boiler, under the archway usually constructed in front of each boiler. By a line of rail carried along the outside of the boiler-house the loaded railway wagons are brought up alongside the coal-boxes in succession, and, by a simple contrivance, the coal is discharged from the wagon into the coal-box. The coal-boxes

---

[1] Minutes of Proceedings Inst. C.E., vol. lxx., Pl. 2, Figs. 3 to 6.
[2] *Ibid.*, vol. xxxvii., p. 369, and xxxix., p. 126.

hold 120 tons in all, from twelve wagons; a supply which lasts five or six days, and provides against irregularities of arrival. A saving of £120 annually has been effected by the adoption of this arrangement for economising labour and keeping the fuel in good condition. Saarbrück coal, it is known, after long lying in heaps, sensibly loses its heating power.

D. K. C.

The Purification of Feed-Water for Steam-Boilers.

By Baron de Derschau.

(Comptes rendus de la Société des Ingénieur-civils, August 1882, p. 115.)

Magnesia, whilst it is almost insoluble in water, precipitates the bicarbonates, and is itself reduced to the state of a neutral carbonate. Under these conditions it reacts quickly, and at any temperature, on sulphate of lime in precipitating carbonate of lime, sulphate of magnesia being left in solution. For waters in which sulphate of lime predominates, basic carbonate of magnesia is added, and it reacts in the same manner on the sulphate of lime.

This system of purifying feed-water for locomotives is employed on a large scale in Russia, on 11,000 miles of railway. Previously the engines, after having run from 8,000 to 11,000 miles, were, in consequence of the impurity of the water-supply, laid up for heavy repairs. On the other hand, on lines where the water was purified according to the magnesia process, the engines ran nearly 75,000 miles before requiring heavy repairs. Engines, after having run upwards of 4,000 miles without priming, being simply blown off at intervals of ten days, were free from incrustation, and had but a soft and non-adherent deposit of magnesia. The cost of the purification of the feed-water was at the rate of 1·46 penny per cubic yard, or 0·153 penny per mile run.

The magnesia process is conducted in tanks holding 4,400 gallons, and may be performed three times per day. The tank is charged with a quantity of hydrated magnesia sufficient for twenty operations. By means of a Korting injector, an intimate mixing is maintained for thirty minutes; and at the end of four hours' rest the water, freed from calcareous salts, is decanted without filtration, into the water-tank for the service of the engines. To render the operation more nearly automatic, apparatus has been employed, consisting of four iron cylinders, which, for the supply of 22,000 gallons in twenty-four hours, are 24 inches in diameter, and 3¼ feet in height. These are filled with a filtering medium composed of sawdust and hydrated magnesia, through which successively the water is conducted from top to bottom, and whence it passes off purified; all the carbonate of lime being precipitated and deposited on the superficies of the sawdust, in the state of crystals of arragonite, so that the sulphate of lime is replaced by sulphate of magnesia. The cylinders are cleared and fresh-charged as required.

The compactness of this apparatus is evident when it is considered that, in order to purify 100,000 gallons in twenty-four hours by the tank process, at least seven tanks are required; whereas, by the filtering process, four cylinders 3 feet 9 inches deep and 4 feet in diameter would suffice for the same rate of production.

D. K. C.

Purifying Feed-water Heater. By G. S. Strong.

(Journal of the Franklin Institute, November 1882, p. 321.)

According to the results of the investigations of Dr. Joseph G. Rogers, it appears that the high temperatures necessary to heat water through the medium of thick scale, sometimes cause the conversion of the scale into a species of glass by the combination of sand, mechanically separated, with alkaline salts, and that the conducting power of such incrustations is to that of iron plate as 1 to 37½. He estimates that, in consequence, a scale $\frac{1}{16}$ inch thick requires an additional expenditure of 15 per cent. of fuel, the expenditure increasing as the scale grows thicker.

The Author of the Paper has designed a feed-water purifier for the separation of all matters, soluble and insoluble, from the water, by means of which water is passed into the boilers quite free from any substance that would cause scale. He perceived that all substances likely to give trouble by deposition, would be precipitated at a temperature of about 250° Fahrenheit. He therefore, after having raised the temperature, by means of the exhaust steam, to from 208° to 212° Fahrenheit, introduced a coil of live steam from the boiler, to raise the temperature still further to 250° Fahrenheit. By the adoption of this method the chemical purification, which would otherwise take place in the boiler, takes place in the heater, and the heated water is finally passed through a filter of wood-charcoal and bone-black on its way to the boiler. A feed-water heater on this construction, applied to a locomotive, proved to have effected a saving in coal of 22 per cent., and an increase of evaporative efficiency of 1·09 lb. of water per pound of coal.

D. K. C.

Nydquist's Locomotive for Secondary and Branch Lines.

(Organ für die Fortschritte des Eisenbahnwesens, vol. xix., 1882, p. 181.)

This is a light locomotive, with tender and goods-van combined. It was designed by Mr. Nydquist, with the object of reducing the dead load to a minimum, for lines with small traffic. To ensure a regular and uniform motion, all projecting parts are avoided, and the axles placed as far apart as possible. The weight of the dead load is concentrated on one pair of wheels, the second pair carrying the live load. The engine is constructed for a line of 891 millimetres (35 inches) gauge. The chief dimensions of an engine of this gauge are as follows:—

Diameter of cylinders 180 millimetres . = 7·1 inches
„ „ driving-wheels 900 millimetres = 35·4 „
Wheel-base . . . . = 9 feet 2·2 inches.
Steam-pressure . . . . = 10 atmospheres.
Speed per hour . . . . = 18½ miles.
Capacity of bunkers . . . . = 440 lbs.
„ „ water-tanks . . . . . . = 176 gallons
Weight (loaded). . . . . = 9 tons.

The space set apart for the storage of goods can be heated from the fire-box, thus protecting, during the winter, those goods which are liable to suffer from the effects of cold. Seats are also provided, in case the space should be required for the accommodation of passengers.

Only one man, with the occasional assistance of the guard in extreme cases, is required to manage the engine.

These locomotives have also been constructed for a 4 feet 8½ inches gauge. Their cylinders then vary in diameter from 200 to 220 millimetres (7·9 to 8·7 inches), their driving-wheels having a diameter of 1·0 to 1·1 metre (39·4 to 43·3 inches); they attain a speed of 35 to 40 kilometres (22 to 25 miles) per hour, and weigh, when loaded, 13 tons.

The cost of an engine of the narrower gauge is 14,625 marks (£716), and of one of 4 feet 8½ inches gauge, 16,875 marks (£826).

J. R. B.

Fireless Locomotives. By — LENTZ.

(Wochenschrift des Vereines deutscher Ingenieure, 1882, p. 359.)

The fireless locomotive, the subject of Mr. Lentz's Paper, is designed for use on tramways. It is constructed so as to differ as little as possible, in its external appearance, from the cars attached to it, and is otherwise, except for the absence of a fire-box, similar to a locomotive of ordinary construction.

The boiler, or reservoir, is filled to about three-quarters of its capacity with water; steam of 17 atmospheres from a stationary boiler is then supplied to it. The engine-boiler is thus filled with steam of from 15 to 16 atmospheres; this is expended on the journey, until the pressure is reduced to 2 to 3 atmospheres. By means of a valve (*détendeur*) the steam is expanded to any degree requisite at the time; but, before entering the cylinders, it becomes superheated, by passing through a tube covered by the water in the boiler.

To protect it from cooling, the boiler is well encased, the loss of pressure amounting to only ½ atmosphere in winter, and ¼ atmosphere in summer. The feeding boilers are placed, as far as this is possible, at points along the line having the least elevation, so that the engines may ascend the gradients whilst possessing their maximum steam-pressure.

Several of these fireless locomotives are running in Marly and Lille, and twenty have been ordered for a line in course of construction in Batavia.

J. R. B.

Locomotives Constructed for Railways in France, 1878–81.

By — Deghilage.

(Revue générale des Chemins de Fer, 1882, p. 237.)

For the first time, orders for rolling stock, especially locomotives, for French railways, have been executed in part by foreign contractors, in order to supply the demands of the railway companies. During the four years 1878–81, the number of locomotives delivered or ordered amounted to two thousand three hundred and forty-four, of which one thousand eight hundred and twenty were ordered in France, and five hundred and twenty-four in England, Austria, and Belgium. Of the engines constructed in France, four hundred and forty-nine engines, or 25 per cent. of the total number, have been constructed, or are to be constructed, by the railway companies. The average price of the engines of French construction is 7·13*d.* per lb., and that of foreign engines, including freight and duty, is 6·91*d.* per lb.

The express engines of the Northern, the Southern, and the Orleans Railways, have remained without modification since 1878, when they were represented at the Paris Exhibition. But the designs of those of the three other companies, the Eastern, the Lyons, and the Western, have since been revised. For the Eastern Railway, ten engines, constructed in 1878, had fire-boxes of the Belpaire type, and 7½-feet wheels. In the more recent engines, the fire-box shell is circular, with transverse roof-stays, with 6 feet 10 inch wheels. For the Lyons Railway, the engines of 1878 had 6 feet 10 inch wheels, and outside valve-gear, weighing 45 tons in working order, of which 25½ tons was driving weight. The more recent engines have wheels 6 feet 7 inches in diameter, coupled, with Allan's valve-gear inside, and weigh 48 tons, of which 27 tons is driving weight. For the Western Railway, the wheels have been increased in diameter from 6 feet 3 inches to 6 feet 7 inches, and the valve-gear has been brought inside. It does not appear that the bogie has been employed in any engines but those of the Northern Railway.

The specialties of the locomotives for several railways are described and illustrated in detail, and a general table of dimensions and other particulars is appended to the Paper.

D. K. C.

Pouget's Chronotachymeter. By A. Brüll.

(Portefeuille économique des Machines, 1882, col. 146.)

The above instrument is for automatically recording, not only the exact time of departure and arrival of a train at every station, but also its direction and speed at all times, and at every point of the journey. It consists of a small disk, or roller, with rounded rim, which is kept constantly pressed upon the tire of a trailing wheel of the locomotive by means of a pair of springs. The roller thus derives a motion which is independent of the size of the wheel, and is exactly equivalent to the distance passed over by the train. On the axis of the roller is a worm, gearing with a worm-wheel. The latter by this means makes two revolutions in a distance of 25 metres, and causes a system of levers to act on a ratchet-wheel in such a way as to ensure the movement of two hammers. One of these hammers impresses a small straight mark on a sheet of paper at every 25 metres; the other, a triangular mark at every kilometre. By an ingenious arrangement, a third series of marks, viz., a row of small circles, is made, as long as the engine is moving backwards. Yet a fourth kind of mark, in the form of a straight cross-band, can be made by the station-master when he gives the order to start the train.

All these marks are made on a sheet of paper, which thus becomes an unerring way-bill (*feuille de route*). This sheet is wound round a cylinder, which derives its motion from an accurate clock. Its axis is a screw, along which it at the same time moves with a helical motion. Inasmuch as only a strip is required whereon to make the marks, no less than ten series of these can be recorded side by side. Each turn takes fifteen minutes (fifteen divisions being made across the paper), so that the paper lasts two hours and a half. A specimen of the record taken June 20, 1882, between Paris and Havre, is given, and its interpretation is easy and of interest.

After criticising the invention with regard to some minor details of wear, scale of the marks, &c., the Author concludes by saying that the "Pouget Chronotachymeter solves, in a more complete and satisfactory manner than the various kinds of apparatus hitherto proposed, the difficult problem of the automatic registration of the movement of locomotives; and this invention ought to be carefully tried by railway companies. There is no doubt that with the improvement in detail, which practice is certain to bring about, this ingenious instrument will soon constitute one of the important elements of regularity and security of working."

H. S. H. S.

Uniformity in Railway Rolling-Stock. By O. CHANUTE.

(Transactions of the American Society of Civil Engineers, vol. xi., 1882, p. 291.)

The object of this Paper is to point out the great economy and advantage which result from the construction of railway rolling-stock with absolutely interchangeable parts. The case of the Erie Railway (now the New York, Lake Erie, and Western) is selected for discussion. There were, in 1874, locomotives on the line which constituted eighty-three different types of engines, and comprised the following varieties of styles of parts exposed to breakage, and involving the necessity for duplicates in stock: Of cylinders, seventy styles; crank-axles, fourteen styles; smoke-stacks, seventeen; smoke-box doors, forty-one; driving-wheel axle-boxes, seventy-one; driving-wheel springs, forty-two; expansion-links, twenty-five; eccentrics, thirty-two styles; and so on.

Of cars, there were eleven thousand seven hundred and forty-four in 1874, consisting of two hundred and thirty varieties, comprising twenty-seven styles of headstocks, nineteen styles of journals, fifty-three styles of oil-boxes, fifty-two styles of brake-shoes, and so on.

In 1876, the management, whilst reducing the gauge of way from 6 feet to 4 feet 8½ inches, embraced the opportunity to reduce the rolling-stock to a few standard types. For this purpose they introduced a new type, that of the "Consolidation" pattern of locomotive, which was twice as powerful as those of the ordinary "American" pattern, and admitted of the train-loads being largely increased. Sixteen new Consolidation engines were constructed according to the same working drawings by the Company, and by two firms of builders. It was thought that all these would be interchangeable; but, in the course of a few months, this illusion was dispelled by a "butting collision," in which one of the engines had the front end stove in. It was attempted to apply the sound front end of another of the engines, but the bolt-holes mismatched by an eighth of an inch, just enough to prevent a good fit. The drawings had not provided for the "personal equation" in construction. It was consequently determined to supply templates to each contractor, provided with hardened steel bushings to guide the drill in boring holes. These templates are thirty-nine in number, and there are now one hundred and eight Consolidation engines on the road, so exactly built that duplicates, sure to fit, can be kept in hand in case of accident. There will ultimately be but eight types of locomotives on the line, instead of the eighty-three varieties in existence in 1874, and the economy already achieved has been very marked. In the years 1870–74, previous to the commencement of the alterations, the cost for repair of the locomotive stock averaged 4·59*d.* per mile-run. In the last five years, 1877–81, the cost was only 2·17*d.* per mile-run, showing a saving of more than half the previous cost. This economy is, no doubt, in part due also to reforms of management, improvement of the way by the substitution of steel rails for iron rails, lower wages, and to the fact that many of the engines are comparatively new.

The cost for repair of cars, treated in the same way, has been reduced from £161 to £91 per passenger-car per year, and from £18 to £8 10*s.* per freight-car per year. The freight equipment had been very much increased during the last three years, and the new cars had not been sufficiently worn to arrive at their normal state of repair.

In the department of bolts and nuts used for fastenings of cars, it is most important, for promptness and economy of repairs, that the bolts and nuts should be absolutely uniform and interchangeable. The railroad company had, in 1873, nominally adopted the "United States" system and standard of screw-threads, of which a number of sample taps and dies were provided, to be renewed when worn out in the workshops. A gradual divergence from uniformity took place, nuts cut at some shops did not properly fit bolts of the same diameter cut at other shops. Neither the diameters, nor the pitches, nor the depths of the threads, nor their angles, nor the flat surfaces, were found to agree. The differences in diameters varied from $\frac{17}{10,000}$ up to $\frac{550}{10,000}$. Each shop, in reproducing its taps and dies, had, by imperceptible degrees, departed from the standard. Three-quarter inch nuts taken at random from the car stock of sixteen other companies, exhibited the same tendency to enlargement, most of them having attained a diameter of 0·7815 inch. A reversion to the original standards was resolved upon, and it has been found more accurate and less expensive to buy taps and dies than to continue their manufacture.

D. K. C.

Endless-Rope Tramways in Chicago. By D. J. Miller.

(Journal of the Association of Engineering Societies, October 1882, p. 397.)

The city of Chicago is supplied with a system of tramways, above four miles in length, in which horse-power has been superseded by the tractive force of wire ropes, worked in tunnels by stationary steam-power. The carrying capacity is comparatively unlimited, the State Street line, now in operation, being competent to carry ten thousand passengers per hour. The lines in the Wabash and Cottage Grove Avenues were to be opened in the end of November 1882, and to receive motive power from the machinery employed in working the State Street lines. The cables for working these lines are carried some distance from the engine-house before being brought into actual service. Cottage Green Avenue line commences at a distance of 1,920 feet from the engine-house. This line runs south for a length of 11,642 feet to the wheel-vault, and returning it makes a total length of rope 27,124 feet. The Wabash Avenue rope, going and returning, is 22,873 feet long. These two cables are carried from the engine-house to their respective lines through a third tunnel between the lines of way. At each angle in the line, a 12-feet sheave is laid underground, about which the rope is passed, and by which the rope is maintained in a direct line from one wheel-vault to another.

In order to obviate shunting devices, a belt-line has been laid, on which State Street cars pass east on Madison Street to Wabash Avenue, then north to Lake Street and west to State Street, returning south, and completing a circuit of three blocks. The belt-line is worked by an auxiliary cable which is propelled by the end wheel upon which the State Street main rope is reversed: the main rope being carried into a vault and passed round a 12-feet sheave, whilst the auxiliary rope is passed round a 6-feet sheave on the axle of the larger sheave, and derives its motion at half the speed of the main rope. The auxiliary rope is kept taut by passing it round a loaded sheave.

The tunnel is 3 feet in depth, below the paving; and has an average width of 15 inches. It is constructed (apparently, from the illustration) in concrete, in which are embedded strong angle-irons of a V form at intervals, which form a sort of framework for the tunnel, and are at their extremities, bolted to the two rails and longitudinal timber sleepers which form the way. At the upper part of the passage formed between the limbs of the V an inner angle-iron frame is inserted, and riveted to the limbs, and supports the middle slot-rail, which is of cast-iron, and is bolted to it. The rope is supported on pulleys at a level 21 inches below the surface, and, when in grip, it is 13 inches below the surface, making 8 inches of difference of level, for clearance. The ropes are of steel, 1¼ inch in diameter, having a breaking stress of 39 tons. The working stress, calculated by dividing the indicator HP. by the speed of the cables, is 5½ tons; and, adding half the straining weight, 6½ tons.

On a road properly constructed, with duplicate cables to provide for breakage, it is considered that a gain of from 30 per cent. to 50 per cent. may be effected, when from two hundred to three hundred horses would be required on the ordinary system; and from 60 per cent. to 70 per cent. on larger systems.

It is stated that the proposition to draw carriages on steel railways by means of an underground endless-rope, connected to the carriages through a continuous slot, is not by any means a new one, but was proposed as long ago as 1858 in a patent granted to E. S. Gardner of Philadelphia. A sketch of this plan is given.

D. K. C.

Raffard's Friction-Dynamometer. By — COLLIGNON.

(Bulletin de la Société d'Encouragement, June, 1882, p. 269.)

Raffard's dynamometer is an improvement of Carpentier's dynamometer. On the driven shaft, three pulleys are placed, of which the central pulley is keyed on the shaft, and the two outer pulleys are loose. The three pulleys are spanned by a bridle, parallel to the axle, to which three flat bands are connected, one band to each pulley. The middle band passes over the middle pulley, and sustains a weight at its lower end. The two outer bands pass under the outer pulleys, one to each pulley, and are carried up to a bridle on the end of a horizontal lever overhead, to the extremities of which they are connected. To the other end of the lever a counterweight is attached, which balances the smaller weight that hangs from the driving pulley, plus the frictional resistance between the driving pulley and the band. The difference of the weights is the measure of the frictional resistance from which, with the velocity, the power is calculated.

D. K. C.

Coal-Dust as an Element of Danger in Mining, shown by the Explosion in the Albion Mines, November 12, 1880.

By C. HOVEY.

(Proceedings of the American Association for the Advancement of Science, 1881, p. 74.)

The main seam of these mines is 37 feet 6 inches thick, and is highly bituminous. It has been worked continuously since 1807. The earlier workings were abandoned in 1839 on account of a fire, and a new opening, the Bye pit, was worked till 1863, when a fire occurred from a shot lighting gas, and the pit had to be closed up. The Foster pit was next opened; but in 1869 spontaneous combustion of slack caused a fire which necessitated its abandonment.

The explosion took place in the Foord pit, which was ventilated by a large Guibal fan, circulating 120,000 cubic feet of air per minute. This pit was considered remarkable for the completeness of its ventilation. On the morning of the disaster the night watchman reported the mine free from gas except in small quantities; but within an hour afterwards a series of explosions began, which continued at intervals until the whole mine was a furnace. The explosion took place at the moment the workmen entered the dips. It was first noticed at the fan-shaft, and a minute later at the drawing shaft, having in one case travelled with and in the other case against the ventilating current. The locality where the workmen were known to be was 1,200 yards south of the drawing-shaft, and the exploring party penetrated 600 yards in that direction. It was evident the flame had not reached so far, as the splintered woodwork was not charred; but the walls looked as if swept by a broom, and were clear of timber. Volumes of dust lay on the floor and clouds of finer particles were swept on into the north-side levels. At the lamp-cabin an open light was always kept burning, as it was considered safe; but here a secondary explosion took place, not, however, extending far into the north side. Secondary explosions, caused by generated or extracted gas, are usually near the primary one; but in this case the second was half a mile from the first, with an intervening space of a quarter of a mile, known to be free from gas. The ignition of these volumes of dust did not harm the shafting, because it was wet, and the flame was extinguished as soon as it touched the damp walls. Elsewhere the mine was a very dry one.

When attempts were made to restore ventilation the mine was discovered to be on fire.

Comparing the above facts with the experiments of Galloway and Abel, the Author concludes:

1. That coal-dust, under favourable circumstances, becomes the vehicle of flame.
2. That it may spread and augment gas-explosions.
3. That it may determine and precipitate explosions due to the presence of inflammable gas in otherwise harmless and inappreciable quantities.
4. That it would be a wise precaution to water dusty galleries, which would at least tend to reduce the range of explosions.

E. H.

On the Gases Separated in Steel-Castings. By F. C. G. Müller.

(Stahl und Eisen, vol. ii., p. 531.)

The Author in this Paper replies to the criticisms on his former work contained in Mr. Pourcel's Paper, read at the Vienna meeting of the Iron and Steel Institute, and to the remarks of Mr. Windsor Richards upon it. After pointing out that the cavities in unsound ingots are not the result of the enclosure of gas-bubbles in a liquid, but tubes resembling those of worms produced by the separation of gas from previously solidified metal, having their longer dimension perpendicular to the cooling surface, he states that two theories of their origin have been proposed. The first of these, called the absorption theory, supposes that the fluid metal takes up gases from the air or the fire, which are given out again on solidification and cooling. Such an action is known to take place with silver and copper, and an analogous action is seen in the freezing of water, where the dissolved air separates and forms cavities very similar in appearance and radial arrangement to those of an unsound steel ingot. It is not necessary to suppose that the whole of the gas so taken up is separated, a portion being retained at a temperature far below that of solidification. In this respect the first separation of gas represents the first crystallization of mixed salts from a solution, and the residuum, the salts retained by the mother liquor.

The second theory, characterised by the Author as the reaction theory, supposes the separated gases not to be previously contained but to be formed at the moment of separation by the reaction of dissolved oxide of iron upon carbon. This requires the gas in the pores to be of a definite composition, namely, carbonic oxide, while in the absorption theory no particular composition is specified. The part may be played by carbonic oxide, as well as by any other simple or compound gas.

The second theory is considered by the Author as opposed to many observed facts. For instance, completely decarburized and overblown metal, when cast without any additions, gives off more gases in setting than any other kind, their volume being many times that of the metal, which cannot be due to reaction, as there is no carbon present.

Mr. Pourcel's explanation of the effect of silicon in producing sound castings, by its reducing action on dissolved oxide of iron, producing silica instead of carbonic oxide, is contrasted with the circumstance that German Bessemer steel is rich in silicon, and yet unsound ingots are common. Part of the Author's experiments were made with steel containing 0·3 to 0·6 per cent. silicon and 0·8 per cent. manganese, and in the direct Bessemer process, steel with 1 per cent. of silicon is often produced, which rises so quickly that there is often scarcely time to stopper the moulds. Another fact is that in recarburizing highly silicized steel with spiegeleisen, a violent evolution of carbonic oxide takes place, showing that the reaction of carbon upon oxide of iron is effective even when the bath contains a notable quantity of silicon. In some cases the Author has found an actual increase of silicon after the addition of spiegel or ferro-manganese, showing that only the carbon and manganese of these compounds were effective as deoxidizing agents. Again, in the ordinary German Bessemer process there is no sensible oxidation of silicon so long as carbon is present, its behaviour being, in fact, similar to that of phosphorus in the basic process.

Although, therefore, there is no ground for supposing that the mere presence of silicon in the metal hinders the formation of gas pores, it is otherwise when an addition of a silicon compound is made before casting. This the Author considers to be a proof of the accuracy of the absorption theory, as showing the phenomenon to be merely physical in its nature. Silicon increases the solvent power of steel for gases, so that the bath, when previously saturated, becomes only partially so by the addition, and therefore separation is prevented. Of course, if the opportunity is given, it may become saturated a second time, and its solvent power may be increased by a second addition of silicon, and so on. The latter statement is only put forward as a possibility, as there is no experimental evidence to show whether the solubility for gases is proportional to the content of silicon. It is more likely that an addition of 0·1 per cent. to a bath containing none may be more effective than an increase of an original 0·5 to 0·6 per cent. The first case is that of the Martin process, where highly silicized compounds are most effective. This simple explanation, the Author points out, has not only been previously published by himself, but was given by Mr. Gautier a year since, at the meeting of the Iron and Steel Institute.

That small additions of foreign elements materially influence the solubility of gases in metals is proved by the analogies of copper and nickel. These metals, which, when chemically pure, give porous ingots from the separation of carbonic oxide and hydrogen; cast perfectly sound when an addition of 0·1 per cent. of lead is made to the first, and of 0·12 per cent. of magnesium to the second when melted. It seems, therefore, extremely probable that silicon may have a specific action upon iron of a similar character.

The Author then reviews the various analyses of gases given off by steel, commencing with those of Troost and Hautefeuille, in 1873, Parry's in 1874,[1] Regnard's in 1877, and his own in 1878–79. These are repeated at length, on the ground of their having been imperfectly reproduced in foreign journals.[2] The general result of the whole of these is to show that the gases liberated, whether by heating the metal in vacuo or boring it, consist essentially of 85 per cent. hydrogen and 15 per cent. nitrogen. Mr. Stead's experiments, made for Mr. Windsor Richards in 1880, give an essentially similar composition. The supposition of the latter observer that the hydrogen may be due to the decomposition of water by the friction of the metal against the borer, is shown to be untenable, as the shavings taken off when steel was slowly bored in air showed no tendency to tarnish, and if, therefore, there was no disposition to take up free oxygen, it is not likely that they would decompose water in a specially cooled apparatus in order to become oxidised. Even such a tendency, if it existed, would be checked by the reducing atmosphere produced when the first few cubic centimetres of hydrogen had collected in the bore-hole. The enormous volume of gas, removed in Stead's experiment with the blunted borer, amounting to about eleven times that of the metal, shows that the whole of the intermolecular gas has been removed, owing to the metal being practically pulverised. This, however, is in accordance with the fact previously pointed out by the Author, that the amount of gas removed depends upon the thickness of the shaving. That the volume of these intermolecular gases should be so large is, however, a new and important discovery. Parry, by heating in vacuo, obtained only a three- to

---

[1] This should be 1872.

[2] The account is the same as that given in the Min. Proc. Inst. C.E., vol. lx., p. 499.

four-fold volume. The Author considers that these gases are actually alloyed in the steel; that nitrogen is actually so contained is proved by the admirable experiments of Allen, and he is now at work upon a method for the quantitative determination of hydrogen. Both elements are integral constituents of the steel, and possibly may exercise an important influence upon its physical proportions.

Carbonic oxide is also contained in the intermolecular gases, but as it is only given off at a red heat, and not in the boring experiments, it is evident that it forms a more stable combination with the metal than either hydrogen or nitrogen.

In conclusion, the Author points out that the results of some fifty experiments, made by different observers in different ways, show that hydrogen is the principal gaseous constituent obtainable from steel, whether in the gases from the molten metal, the contents of the cavities in the ingots, or the intermolecular gases of the compact metal. Nitrogen is a never-failing associate, while carbonic oxide is invariably subordinate in importance to hydrogen, and is never found in the larger cavities. It is most probable that these gases are due to the decomposition of steam in the air or gases of the furnace, and that their effect can be neutralised either by the addition of silicon, so as to increase the solvent capacity of the metal, or that they can be separated mechanically by the rapid evolution of another gas on the addition of spiegeleisen.

H. B.

On the Cause of the Separation of Copper from Lead by Liquation. By A. Schertel.

(Jahrbuch für das Berg- und Hüttenwesen in Königr. Sachsen, 1882, p. 176.)

The mixed character of the ores smelted at Freiberg results in the production of an impure furnace lead, which requires a preliminary refining operation before it can be desilverized by the Pattinson process. This consists in liquation in a small reverberatory furnace, having an inclined bed and a receiving pot for the liquated lead. The ore-furnace lead, containing from 0·4 to 1·0 per cent. of copper, is charged upon the bed at the fire-bridge end, and gradually melted down at as low a temperature as possible, when a separation takes place into soft argentiferous lead, which runs down the hearth into the receiving-pot, and a less fusible dross, *saigerdörner*, which remains in the furnace. The proportion of the latter is from 2 to 5 per cent. of the weight of the lead charged, and it keeps back from 85 to 95 per cent. of the original copper contents.

The separation is not, however, confined to copper, as other metals are also removed, at least partially, as will be seen from the following analyses, which represent the results obtained in the treatment of 333 cwt. of furnace lead, containing—

	Per Cent.		Per Cent.
Silver	0·544	Tin	0·210
Copper	0·940	Nickel and cobalt	0·055
Bismuth	0·066	Iron	0·027
Arsenic	0·499	Zinc	0·022
Antimony	0·820	Sulphur	0·200

By liquation, the above quantity of lead gave 16·2 cwts., or 5·1 per cent. of its weight of coppery dross of the following composition—

	Per Cent.		Per Cent.
Silver	0·17	Tin	0·04
Lead	62·40	Nickel and cobalt	1·09
Copper	17·97	Iron	0·43
Bismuth	..	Zinc	0·07
Arsenic	2·32	Sulphur	4·00
Antimony	0·98	Oxygen	1·87
		Total	91·34

The remainder consisted of slag, hearth-stuff, and ashes.

From this it appears that the whole quantity of sulphur, 96 per cent. of the nickel and cobalt, 93 per cent. of the copper, and 25 per cent. of the arsenic, are separated in the liquation residues, which retain only 5·8 per cent. of the antimony, and 1·54 per cent. of the silver. The whole of the bismuth and the tin, to within 0·9 per cent., remain in the liquated lead.

The residues, when melted with borax, separated into three distinct products: a metallic mass below; an arsenical regulus, or speiss, in the middle, and a sulphuretted regulus above. It might be supposed that these were original products formed in the ore-furnace, and interspersed through the lead, the separation taking place subsequently at the low heat of the liquation furnace; but in that case the composition of the sulphur and arsenic compounds should be similar to those obtained in ore-smelting. That this is not the case is evident from the analysis of the upper regulus, which contains—

	Per Cent.
Sulphur	17·72
Lead	32·80
Copper	47·70
Arsenic	1·15
Nickel	0·35
	99·62

Whereas the regulus obtained from the ore furnace always contains a notable quantity of iron.

The Speiss contains—

	Per Cent.
Lead	25·68
Copper	37·60
Nickel	8·60
Arsenic	27·00

or almost the entire quantity of the latter element contained in the residues. The remarkable affinity of arsenic for nickel and cobalt is also rendered strikingly apparent; the entire contents of these metals, which exist in the lead in only very minute quantities, being concentrated more than 150 times in the speiss.

The third or metallic product contains:—

	Per Cent.
Silver	0·34
Copper	1·79
Arsenic	0·75
Nickel	0·08
Lead	96·50

Copper and lead being in the ratio of 1 : 16 atoms. This alloy substantially represents that liquated from the copper cakes in the old process of desilverizing coarse copper by means of lead.

From the Author's results, given above, it will therefore be apparent that the success of the process depends not on the formation of a comparatively infusible lead and copper alloy, but on the presence of sulphur and arsenic in the furnace-lead; the first tending to the production of a coppery regulus, and the latter, probably from the presence of a small quantity of nickel, of a speiss at the low heat of the liquating furnace.

H. B.

Generator-Furnace at the Pforzheim Gasworks. By H. Brehm.

(Journal für Gasbeleuchtung, 1882, p. 558.)

This furnace heats six D-shaped retorts. The width of the oven is 2·6 metres (8 feet 7 inches), and the flues for heating the air-supply have a superficial area of 40 square metres (430 square feet). The furnace is worked with steam produced by the heat of the waste gases; 70 per cent. of steam is used for every 100 per cent. of coke burned. The furnace only requires a draught of 2 to 3 millimetres (0·079 to 0·118 inch). Each retort carbonizes 275 lbs. of coal in four hours, and produces 30 cubic metres of gas for every 100 kilograms of coal used (= 10,780 cubic feet per ton). The fuel required is 11 kilograms of coke per 100 kilograms of coal. The fire is clinkered once every thirty-six hours.

The residue consists of . . .	5·25 per cent.	coke,
	5·46 „	clinker,
	5·17 „	ash.
Total . . .	[1]14·88 per cent.	of the original coke used.

The fuel required for 100 kilograms of coal is therefore reduced to 9·36 per cent. of pure carbon.

---

[1] *Sic* in original.

It has not been found necessary to work the oven to its maximum capacity, but the Author expresses the opinion that the retorts might be made to produce 300 cubic metres (= 10·590 cubic feet) per diem, without increasing the percentage of fuel used.

G. E. S.

The Purification of Gas by Artificial Oxide of Iron.

By — Lux.

(Journal für Gasbeleuchtung, Sept. 1882, p. 589.)

The useful effect of oxide of iron in purification has been theoretically shown in Schilling's Handbook to amount, in some cases, to 50 or 55 per cent. In general practice it is much less, the Author estimating it at 20 to 25 per cent. of what might theoretically be expected. The reason of this is to be found in the inferior character of the oxides hitherto used. These are of four types, viz.:—

1. Burnt pyrites.
2. Natural ores, bog-ochre, &c.
3. The refuse from the manufacture of aniline.
4. Artificial oxide, prepared with iron borings by Deike's method.

The oxide remaining from the burning of pyrites, owing to the high heat to which it is exposed, exists in a "dead" burnt condition. It can with difficulty be dissolved in sulphuric acid, and will act but slightly upon sulphuretted hydrogen. It contains about 10 per cent. of sulphate of iron, which also has no effect on hydrogen sulphide, except by the addition of an alkali. On account of these circumstances, burnt pyrites has at first no effect in purifying gas, but after a portion of the iron has become hydrated, and the ferrous sulphate has been decomposed by the ammonia in the gas, it begins to remove the sulphuretted hydrogen. The effective power of natural oxides is often diminished by imperfect mechanical division, and by the organic matter present in the material. The refuse from the reduction of nitrobenzol to aniline also contains "dead" burnt or fused ferric oxide, together with ferrous oxide, which, as is well known, combines with a portion of ferric oxide to produce the black magnetic oxide, which has no affinity for sulphuretted hydrogen. The so-called Deike's material, which is principally old used-up oxide renewed with iron borings and sal ammoniac, belongs to the best class of oxides, because the iron is contained in a state of fine division; but it has the disadvantage of being ballasted with about 20 per cent. of sulphur. The axiomatic statement that the purifying power of an oxide is in proportion to the percentage of iron it contains cannot be maintained. The condition of the iron, whether finely divided or not, whether in the form of ferric or of ferrous oxide, whether hydrated or otherwise, is of far more importance than the absolute proportion of iron present.

The best oxide must have—1st, a high percentage of iron; 2ndly, the iron in the finest state of division, and in the form of hydrated peroxide; and 3rdly, must have no impurities mixed with it that detract from the purifying effect of the oxi-hydrate.

The Author prepares an oxide that fulfils these conditions in the following manner:—A natural iron ore, finely ground, is digested with carbonate of soda in a reverberatory furnace, by which a union of the iron oxide and the alumina present in the material with the soda takes place. The melted mass being afterwards treated with water, the iron oxide is precipitated as ferric oxi-hydrate, whilst the impurities, such as alumina and silica, remain in solution. The hydrated oxide is washed until the liquor flowing from it has a gravity equal to 1° Baumé, and is then dried at a gentle heat. The material so obtained contains 70 to 80 per cent. of pure hydrated ferric oxide, in the finest state of division, and about 5 per cent. of carbonate of soda.

In consequence of the fine state of division in which the iron exists, and on account of its alkaline constituent, this material is in the highest degree effective immediately it is introduced into the purifiers. The carbonate of soda assists the purification greatly. It attracts the sulphuretted hydrogen first to itself, and then yields it up to the iron oxide, on which account the material entirely controls the impurity of gases which contain a large proportion of hydrogen sulphide, and which are imperfectly purified by oxides that require considerable time for absorption.

The revivification of this oxide is very rapid, but it will not fire so easily as other oxides, the carbonate of soda acting as a damper to the combustion. The material also works as a mechanical absorbent of other substances contained in gas, such as carbonate of ammonia. With lime, on the contrary, the absorption of the carbonic acid sets free the ammonia, and the gas is rendered again impure.

Lastly this oxide must act on the sulpho-carbon compounds by virtue of the soda carbonate contained in it.

Experience in the use of this oxide has proved its superiority over other oxides. At the Gasworks at Breslau it has been shown to have a purifying value equal to four times that of the oxides formerly used, while the ammonia present in the gas has been greatly reduced. Figures are not at hand to show the effect upon the sulpho-carbon compounds, but the unpleasant smell attending the revivification of the oxide points to the probable volatilisation of these impurities.

The process of manufacture is patented in England and on the Continent.

G. E. S.

Atmospheric Burners for Gas Cooking- and Heating-Apparatus.

By G. Wobbe.

(Journal für Gasbeleuchtung, 1882, p. 619.)

Experiments were made by the Author to determine how the gas could, in such burners, be perfectly consumed. Theoretically the answer is simple enough, *i.e.*, it is necessary to mix so much air with the gas that the requisite quantity of oxygen for complete combustion is contained in the mixture. This quantity, however, varies with the quality of the gas, and the range is very considerable. According to former experiments, published in the "Journal für Gasbeleuchtung," No. 18, in 1880, 100 litres of gas require 220 litres of air to fully deprive it of its illuminating power; and ordinary coal gas is said to require 560 litres of air per 100 litres of gas for its perfect combustion. The percentage of gas and air are as 15 to 85. In issuing with a certain velocity from the orifice the gas creates a corresponding vacuum, which causes the air to follow the same direction. The air and gas mix, and proceed together at a certain speed. It is known that mixtures of gas and air are explosive within certain limits, and that the explosion is propagated with considerable velocity. The speed per second with which explosions of different mixtures fire back has been tested by Professor Mallard.[1]

To determine the greatest velocity with which the gas may be projected from the orifice of the burner, the Author constructed various nozzles for the delivery of the gas, all having the same area at the narrowest part, and measured the consumption of gas at an equal pressure, and calculated therefrom the speed of the current. The nozzles were all of circular form, some having the orifice straight and some taper, some having the narrowest diameter at the end, and others having it in the centre or at the beginning of their length, being widened out again afterwards. Out of seven nozzles,[2] the first four had the narrowest diameter at the exit point, the fifth was contracted at about two-thirds of its length, and Nos. 6 and 7 were narrow at the commencement and bored-out larger for the latter half of their length. These latter had also holes for the admission of air, drilled at right angles to the bore of the nozzles. The experiments gave the following results in litres (0·03531 cubic foot) of gas discharged per minute:—

No. 1.	No. 2.	No. 3.	No. 4.	No. 5.	No. 6.	No. 7.
2·36	2·48	2·57	2·85	2·87	2·91	2·93

The pressure of the gas was 30 millimetres (1·18 inch), the orifices being 1·3 millimetres (0·04 inch) in diameter. The

---

[1] Reproduced in the Journal of the Chemical Society, 1877, vol. i., p. 632.

[2] The illustrations were printed in the wrong order, and have since been corrected in the "Journal für Gasbeleuchtung." No. 7 had the narrowest diameter at the exit point, and those numbered 6 and 7 in the text above were in reality 9 and 10, and were not included in the experiments.—G. E. S.

greatest discharge was given with nozzle No. 7, being 24·15 per cent. more than with No. 1. The velocity, as calculated from the delivery, is 36·71 metres per second. At 20 millimetres (0·787 inch) pressure, the same nozzle gave a velocity of 25 metres per second, and at 3 millimetres pressure it equalled 8·6 metres per second.

To determine the speed at which the mixed air and gas in a Bunsen burner travels, the following formula may be used:

$$V = \frac{M^1 V^1 + M^2 V^2}{M^1 + M^2}, \text{ in which}$$

M^1 = the volume-weight of the gas.
V^1 = the velocity of ditto.
M^2 = the volume-weight of the air.
V^2 = the velocity of ditto.

As the air is, in the first instance, at rest, V^2 will equal 0, and $M^2 V^2 = 0$. The specific weight of gas being 0·44, $M^1 V^1 = \frac{1 \times 0{\cdot}44}{9{\cdot}81} \times 25 = 1{\cdot}12$, in which 25 is the velocity of the gas as determined by experiment at 20 millimetres pressure; and one volume of gas is taken, which may be considered as mixing with 3·33 volumes of air. Then,

$$M^2 = \frac{3{\cdot}33 \times 1}{9{\cdot}81} = 0{\cdot}3394; \text{ and, } M^1 \text{ being } 0{\cdot}0448,$$

$$V = \frac{1{\cdot}12 + 0}{0{\cdot}0448 + 0{\cdot}3394};$$ V therefore equals 2·91 metres per second, if one volume of gas be mixed with 3·33 volumes of air.

The following Table shows the experiments of Mallard in regard to the velocity with which explosive mixtures, containing different proportions of air and gas, fire back, and the speed of the discharge of air and gas in a Bunsen burner calculated by the above formula:

1. Mixture of Gas and Air.		2. Velocity of Explosion per Second in Metres.		3. Speed of Discharge of Air and Gas per Second in Metres, without calculation of the Friction in the Mixing Tube.	
Vol.	Vol.	(a) By Day.	(b) By Night.	$V = \frac{M^1 V^1 + M^2 V^2}{M^1 + M^2}$.	
1	3⅓	0·097	0·194	2·91	
1	4⅓	0·740	1·480	2·30	
1	4¾	0·935	1·870	2·12	
1	5	1·010	2·020	2·02	Apparently cannot be burned without a wire gauze or by the help of compressed air.
1	5¼	0·985	1·970	1·93	
1	5½	0·820	1·640	1·85	
1	6	0·617	1·234	1·70	
1	6½	0·285	0·570	1·58	
1	7½			1·38	
1	10			1·05	
1	14			0·76	

When the backward velocity of the explosion is compared with that of the forward movement of the mixed gas and air in column 3, it is seen that the greatest explosive force occurs with a mixture of five volumes of air with one of gas, the velocity being 2·02 metres per second. This is also exactly the speed of the mixed gases in the same proportion in the Bunsen burner, without allowing for loss by friction. The velocity with which the gases fire back reaches here its maximum; but that of the forward movement of the mixture diminishes continually as the proportion of air increases.

For calculating the reduction due to friction in the internal part of the burner, the following formula is approximately correct:—

$$V = \frac{\sqrt{2gh}}{\sqrt{1 + \zeta + \lambda \times \frac{l}{d}}},$$

in which

ζ = coefficient of resistance at the entrance to the mixing bulb = 0·08;

λ = coefficient of friction in the pipe $= \frac{0 \cdot 0272 + 0 \cdot 0148}{2} = 0 \cdot 021$;

l = length of pipe.
d = diameter of ditto.

In the case of a burner having a tube 11 millimetres diameter, and 110 millimetres long, the loss by friction, allowing 2 metres per second for the original velocity, would be 0·4 metre per second, and the final speed would be 1·76 metre. This shows that it is possible to mix sufficient air with the gas to effect complete combustion without having recourse to compressed air, or to covering the mouth of the burner with wire gauze; an arrangement of which the Author does not approve, as it easily gets out of order, rusts, or becomes perforated.

If the area of the gas nozzle be q, that of the mixing bulb (the air-openings) Q, then, for a burner in which 5 parts of air are mixed with 1 of gas, $Q = \frac{q \times 6 \times 25}{2} = 75\,q$, 25 being the velocity of the gas per second, 6 the number of parts of air and gas, and 2 the speed of the mixed air and gas. It is not possible to construct a burner in which more than 4¾ parts of air are mixed with the gas without danger of firing back; but this proportion is sufficient to cause the gas to burn without smell. Apparatus thus constructed will give an odourless combustion, as well as an increased development of heat and useful effect.

But this construction will not suffice for gases of different quality. Thinner and inferior gas will fire back, and richer gas will cause a smell. Some means of regulation is necessary; this the Author effects by reducing the outflow at the point of combustion.

In some cases he places a conical cap at the end of the pipe, and for other purposes a movable plate, with a slit, is adjusted to the requirements of the gas.

With a properly adjusted burner, boiling vessels can be placed on the top of the burner without producing a smell. The increased effect of thus placing the vessels in contact with the flame is equal to 40 per cent. saving in gas, whilst the increased heat development amounts to 7 or 8 per cent.

G. E. S.

Regenerative Gas-Burners. By F. SIEMENS.

(Journal für Gasbeleuchtung, August 1882, p. 547.)

This is a Paper read before the German Association of Engineers of Gas- and Water-works at Hanover, June 19, 1882.

The idea of the regenerative gas-burner originated in the mind of the Author in connection with the regenerative furnaces, for which he first took out an English patent in the year 1856. The intense brilliancy of the combustion in the furnace, afterwards perfected conjointly by himself and Dr. William Siemens, suggested the possibility of employing the principle to the improvement of illuminating flames, and tentative experiments were at that time made by the Author in this direction, but were not continued, owing to the pressure of other business which prevented his devoting the necessary time to the subject. The first attempts were made in the furnace form, glass being used to give the light free play. This proved to be impracticable, no kind of glass being able to stand the intense heat. After the subject had been allowed to rest for some time, the advent of the electric light impelled the Author to renew his experiments in the same direction. The first attempt which proved really successful was made three years ago, and the burner was erected in Berlin, and described in the gas journals at the time. In this apparatus slit and union burners were used, but later on the Author tried burners of the Argand type, and eventually adopted the form of burner which is now associated with his name. He does not, however, consider that the chief importance attaches to the burner proper, but, firstly, to the particular arrangement of the regenerator, which renders possible the perfect exchange of heat from the products of combustion to the air-supply; and, secondly, to the peculiar cylindrical attachment with the serrated edges that is surrounded outwardly by the flame, and whose inner side forms the flue, and stands in direct connection with the chimney-neck and pipe.

In regard to the exchange of heat, which is the fundamental cause of the photometric result obtained, only a particular and definite arrangement of the regenerator can fulfil the purpose in view. This specialty consists not only in the fact that the two concentric chambers must be vertical, but that the flame must be conducted centrally from above downwards, and the air must travel concentrically from below upwards.

The hot products of combustion, passing downwards, seek the coolest way out; the cold air, on the contrary, in rising upwards seeks the warmest way in order to become lighter. As the middle wall of the regenerating chamber forms the cold surface for the descending heated gases, but the hot surface for the ascending cold air, both currents are impelled as much as possible against the division wall, by which the exchange of heat is greatly facilitated.

The truth of this statement is proved by the fact that, if an ordinary Argand burner be inverted and the chimney connected with some flue, and the flame be made to burn downwards, the glass cylinder will melt, whereas in the usual position it remains comparatively cool. The porcelain attachment is a peculiar feature of the burner. It defines the length and form of the flame, and must be made from some material that will reflect the light, and will not break with the heat.

The ventilation afforded by the burner is a great advantage. The products of combustion pass up a long chimney tube, and may be conveyed away out of the apartment in which the burner is situated.

The regulation of the gas-pressure is important. The Author has devised a governor, or regulator, specially adapted to the requirements of these burners. It consists of an elastic sack or bellows, constructed of thin metal plates, on the principle of a concertina. A cone is attached to the top-plate, which rises with the pressure, thus closing the passage to the burner. In the case of most regulators, the pressure of the gas, acting upon the bottom of the cone, reduces the flow of gas in proportion as the pressure increases. In the Author's regulator this disadvantage does not exist, because the bellows, as they open, present a larger area, and thus counteract the effect of the pressure of gas upon the cone.

G. E. S.

Clamond's Incandescence Lamp. By E. Servrer.

(Journal für Gasbeleuchtung, 1882, p. 580.)

It is through the labours of Audouin and Bérard, led by Dumas and Regnault, that the laws are understood which must be observed to obtain an advantageous development of light by the combustion of gas. The long-known, but hitherto non-utilised fact, that the illuminating intensity of a glowing body increases in a much greater ratio than the temperature, has now been applied by F. Siemens in the construction of his regenerative burners. It is also acknowledged that the heat which is developed by the combustion of gas, is out of all proportion to the small quantity of hydrocarbons contained in it, and which, by their dissociation and the setting free of the carbon particles, are the source of the light produced. It has been even proved that the energy contained in gas can, by being converted into motive power and electricity, be made to produce more light than when burned direct from an illuminating burner, notwithstanding the loss which occurs in converting the heat of combustion first into motive power, then into electricity, and then into light. It is therefore evident that a much greater amount of light could be obtained from gas, if the heat-development of combustion were better utilised.

According to these principles Mr. Clamond has constructed his lamp.

The Clamond lamp is nothing more or less than the Drummond lime light made practically useful. Instead of using oxygen, he uses atmospheric air, and instead of the Zirkon, or lime cylinder, a network of magnesia. Oxygen can in almost all cases be replaced by air, if the latter be strongly heated. Clamond therefore heats the air before it reaches the burner, and obtains a similar effect to that of oxygen. It is not easy to heat the air to a high temperature whilst it travels a short distance. The volume of air passing to the burner is six times that of the gas, and the heat-conducting power is small. In the construction of his burner Clamond causes the air to play upon every part of a fireclay tube, which is heated externally by the combustion of the gas. A pressure of 200 millimetres (7·8 inches) was at first necessary to force the air through this tube, but by improving the construction of the burner, a pressure of 35 millimetres (1·37 inch), is now found to be sufficient.

The network of magnesia, which is heated by the flame to incandescence, is very cheap, and will last for about forty hours, so that it is only necessary to change it once a week. The "wick," as the conical basket-shaped network of magnesia is called, is easily replaced. It is supported by two crossed platinum wires, which are fastened to a brass ring having a bayonet joint.

The light given by the glowing magnesia basket is perfectly steady, and of an agreeable yellow tone. If the 'wick be used longer than for forty hours, the light becomes bluish in colour, and approaches that of the Jablochkoff candle. Also by further use a portion of the magnesia is consumed away, but the small quantity of white dust thus produced is not at all unpleasant or harmful. The magnesia basket is situated at the bottom of the burner to prevent shadows, but the burner can work in any other position, and the light can be regulated by a tap to any desired intensity.

The burner is made in two sizes. One size consumes 180 litres (6·3 cubic feet) an hour, and gives a light of 4·15 carcels (about 37 candles); the other size consumes 500 litres (17·6 cubic feet), and gives a light of 18 carcels (about 162 candles). The burners give a much greater light-effect, in proportion to the gas consumed, than ordinary burners; but it is to be remarked that the ordinary illuminating power of the gas has nothing to do with the light of the lamps, it is the heat of combustion alone that produces it. It therefore follows that it is a matter of indifference whether gas of greater or less illuminating power be used. It would not be necessary with this burner to use gas-coals, but cheap coal might be used, and the cost of production considerably reduced. This is of particular importance for the gas industry, as the use of gas for lighting purposes must in the future go hand-in-hand with that for heating purposes.

G. E. S.

Compressed-Air Feed for Oil-Lamps for Lighthouses.

(Revista de Obras públicas, vol. xxx., p. 13.)

The following arrangement, designed by Dr. Francisco Lizzáraga, has been tried with great success, for a period of three months, in three lighthouses on the Spanish coast, viz., at Machichaco, Cape Mayor, and Igueldo.

The lamp consists of a cylindrical vessel, divided by a diaphragm into two unequal chambers. Oil is pumped into the upper one through a pipe, having its inlet at the bottom of the chamber. This pipe is continued past its connection with the pump, and terminates in a socket secured to the top of the cylinder, and threaded to receive the wick-piece; it is also furnished with a regulating-cock. When oil is fed into the upper chamber, the displaced air is forced through a by-pass from the top to the lower chamber, where it becomes compressed. To light the lamp, the wick-piece is screwed into its socket, and the regulator partially opened. The air-pressure from the lower compartment, acting on the surface of the oil in the upper chamber, then affords a continuous feed.

The respective dimensions of the two chambers are such that the initial pressure in the lower compartment is 1·66 atmosphere. Though it is evident that the lamp must be fed by a constantly diminishing force, it is maintained that the range of the falling pressure is so small, that in practice no evil effects are observed in the illumination from this cause. It is stated that a perfectly steady and constant illumination can be maintained until the whole charge is consumed, provided that after a few hours the opening of the cock is slightly increased to compensate for the loss of pressure.

One of these lamps, charged with 25 litres of paraffin oil, burnt steadily for twelve hours without being touched. At the end of this time the opening of the feed-cock was slightly increased, and the lamp continued burning steadily for four hours more, at the end of which time the oil had been entirely consumed. With a Funck regulator the same lamp burnt for twenty hours.

Another of these lamps, at Machichaco lighthouse, burnt for fifteen hours and forty minutes, with an average consumption of 626 grams per hour. The steadiness of the light was remarkable, until within the last five minutes before extinction. The lamp may be recharged whilst burning by pumping in oil through the discharge-cock, if a longer period of illumination than fifteen or sixteen hours is required. In this case the discharge-cock is screwed to receive the end of the pump-hose.

The principal merits claimed by the inventor for his lamp are its extreme simplicity, and the very small amount of supervision it requires.

E. M.

On the Material Particles in Electric Sparks.

By Dr. Friedrich Wächter.

(Sitzungsberichte der kaiserlichen Akademie der Wissenschaften, vol. lxxxv., March 1882, p. 560.)

In two former communications to the Vienna Academy, Professor Reitlinger and the Author had endeavoured to prove that the glowing particles of the electrode, which constitute an electric spark, emanate entirely from the positive electrode. On the other hand, the experiments of Plücker, Gassiot and others, show that, under certain circumstances, the loosening and projection of the particles takes place at the negative electrode. The object of the present Paper is to discuss this apparent inconsistency, and to consider the difference between the electro-positive and electro-negative loosening of the particles of the electrode.

Only the discontinuous sparks produced by a friction machine, or by the secondary circuit of an induction-coil, are considered, the formation of a continuous electric arc by the heated particles of the positive electrode being well known, in the case of galvanic currents generated by a dynamo machine or ordinary battery. It has generally been stated in text-books, that sparks produced electro-statically possess the same characteristics, whether the discharge be of positive or negative electricity; but the study of Priestley's ring figures, Lichtenberg's figures, and the deflection of the particles by a powerful magnet, show conclusively that the two phenomena are distinct, and marked by essential differences.

The Author summarises the results of his investigations on discharges through a gaseous medium briefly as follows:—

1. The projection of the anode particles takes place in atmospheric air under a pressure of 4,500 to 10 millimetres of mercury; the projection of the particles of the kathode could only be observed for pressures varying from 63 to 0·005 millimetres.

2. The quantity of particles loosened from the electrodes, in equal times and under similar circumstances, diminishes with

decreasing atmospheric pressure for the anode, but increases for the kathode.

3. The anode-particles are projected considerably further than the kathode-particles under the same conditions. (In the case of a pressure of 63 millimetres, about three thousand four hundred times as far.)

4. The anode-particles, uninfluenced by the pressure of the air, always proceed from a relatively small surface; the kathode-particles, on the other hand, are projected from a surface always increasing in area as the pressure diminishes. (The area in the latter case reaches as much as ten thousand times that in the former.)

5. The anode-particles leave always from a determined position on the anode, viz., the point lying nearest the kathode; whereas the kathode-particles proceed from the whole surface of the kathode.

6. The projection of the anode-particles is favoured by an electrode bent or pointed in form; the projection of the kathode-particles by a clean surface, free from oxidation.

7. The direction of motion of the anode-particles is determined by the relative position of the kathode, since they move towards the kathode in the direction of the current along the line of least resistance. The kathode-particles always proceed normally to the kathode surface, uninfluenced by the relative position of the anode and the direction of the electrical current.

8. The anode-particles can move in all conceivable curved paths, while the kathode-particles describe straight lines, and are not capable of moving in a curved line.

9. The anode-particles are deflected by a magnet, as if they were diamagnetic substances; the kathode-particles are affected like paramagnetic bodies.

10. The anode-particles are transferred both in a luminous and non-luminous condition; the kathode-particles only in a non-luminous condition.

11. The anode-particles are of measurable size, and are apparently loosened by some mechanical impulse; the kathode-particles are indefinitely small, and seem to originate from a process of vaporisation.

12. The projection of the kathode-particles is certainly promoted by warming the electrode, whereas this cannot be asserted with safety with regard to the anode-particles.

13. The kathode-particles are the vehicle of the electric current from the electrode to the molecules of the gaseous medium. This is not the case with the anode-particles.

14. From the experiments of Wiedemann and Rühlmann, it appears that a greater electrical tension is necessary for the projection of the anode-particles than is required for the kathode-particles.

These facts prove, in the opinion of the Author, that the difference between the two kinds of electricity cannot be successfully explained by the assumption of a reversal of conditions, which can be expressed by plus and minus, but involves an essentially qualitative change.

E. H.

A Modification of Mance's Method[1] *of Measuring the Resistance of a Battery.* By the late F. J. Pirani, M.A.

(Transactions of the Royal Society of Victoria, vol. xviii., p. 3.)

Dr. Lodge ("Philosophical Magazine," June 1877) has pointed out two defects in Mance's method; firstly, that as only the variation of the current, which is small compared with the current itself, is under observation, a sensitive galvanometer cannot be used; and secondly, although the alteration of the resistance of B D does not change the current in A C, it does alter that in A B, and as the electromotive force of a battery is a function of the current passing through it, this of itself will alter the current in A C. (If the resistance of a conductor varied with the current, Dr. Lodge remarks that it would then be an indefinite quantity, and consequently indeterminable, but on this point the Author observes that though Ohm's law implies that the resistance of a conductor is independent of the strength of the current, yet the latter may produce a change in the conductor itself, *e.g.*, the temperature of a wire or the electric arc, or the chemical composition of an electrolytic cell, thus altering its resistance). To counteract these defects, Dr. Lodge introduces a condenser in A C, and employs a special form of key, by which contact is made for only a very short interval of time; but here, to obtain the greater sensitiveness, the capacity of the condenser, as likewise the resistances of the galvanometer and of the branches A D, D C, must be as large as possible. The Author raises the objection that the current will in this case be very small compared with the strongest the battery can give, but overcomes it by introducing, in place of the galvanometer of the original method, the primary wire of an induction coil, to the secondary coil of which is connected a telephonic receiver, and replacing the contact key by a vibrating spring or microphone, which will only slightly vary the resistance; and

---

[[1] Mance's method is based on the principle of the Wheatstone's balance, where, A B C D being the angles of a quadrilateral connected by six conductors, forming the sides and diagonals; if the current in one diagonal is independent of the resistance of the other, the resistances of each pair of adjacent sides are direct proportionals. As originally proposed, the battery to be measured is inserted in one side (A B), a galvanometer in one diagonal (A C), and a contact key in the other diagonal (B D), and the resistances of the other sides adjusted until, on making and breaking contact with the key, no alteration is observed in the deflection of the galvanometer. The greatest sensitiveness is obtained when the resistances of A D and D C are equal, and likewise A D C and A C, and as small as possible.—F. J.]

though the variation in the current of A C will be also slight, still, owing to the sensitiveness of the telephone, it will readily be perceived. In this way the resistance of a battery can be measured when producing a powerful and practically constant current. This method can also be conveniently applied for determining the resistance of the electric arc.

F. J.

The Variation in Friction produced by Voltaic Polarisation.

By — KROUCHKOLL.

(Comptes rendus de l'Académie des Sciences, Paris, vol. xcv., 1882, p. 177.)

The Author refers to Edison's electromotograph, and to Koch's experiments.[1] The latter showed that polarisation by oxygen increased the friction between the surfaces of the electrolyte and platinum or palladium, but no effect appeared with hydrogen. The Author has repeated the experiment with the same result as regards oxygen, but with decided evidence that polarisation by hydrogen diminishes the friction. The electromotive force of half a Daniell element is sufficient to produce the effect.

P. H.

On Magnetic Screens of Iron. By M. J. STEFAN.

(Sitzungsberichte der Kaiserlichen Akademie der Wissenschaften, vol. lxxxv., March, 1882, p. 613.)

Poisson has shown in his theory of magnetic inductions that if a hollow sphere be magnetised by external forces, the magnetic forces within the sphere, due to the induced magnetisation of the iron, are opposite in direction to the external forces, and that the magnitude of the resultant is very small, provided the diameter of the sphere is not great compared with the thickness of the metal. The Author gives the results of some experiments exemplifying the theory in special cases, and in particular with reference to the influence of a cylindrical iron case in rendering a galvanometer astatic.

A small cylindrical magnet, 3 centimetres in length and 0·4 centimetre in diameter, suspended by a cocoon fibre, had a period of 5·9 seconds, when oscillating under the influence of the horizontal component of the earth's magnetism. A brass cylinder of equal moment of inertia had a period of 34·4 seconds. The magnet was then screened by an iron cylinder, 5 centimetres high, 11 centimetres inner diameter, and 14 outer, placed concentric

---

[1] Wiedemann's Ann., p. 92, 1879.

with the magnet. This reduced the period of oscillation to 13·6 seconds. Since

$$\tau = 2\pi\sqrt{\frac{K}{MT + \theta}},$$

where τ is the period, T the horizontal component, K the moment of inertia, M the magnetic moment, and θ the coefficient of torsion;

$$34 \cdot 4 = 2\pi\sqrt{\frac{K}{\theta}},$$

and $$5 \cdot 9 = 2\pi\sqrt{\frac{K}{MT + \theta}};$$

hence $$\theta = 0 \cdot 03\, MT,$$

and $$\frac{T'}{T} = \frac{1 \cdot 03\, \tau^2}{\tau'^2} - 0 \cdot 03;$$

which, on substituting the values given for T and T', show a diminution of the horizontal component in the ratio of about one-sixth. When the cylinder was raised, so that its centre was about 5 centimetres above the centre of the magnet, the diminution was in the ratio of one-half. With a second cylinder, with walls 1 centimetre thick, the ratio of diminution was one-fifth, when the cylinder was placed concentric with the magnet.

The Author, following Poisson's theory, proceeds to discuss the equations of the lines of force in a hollow cylinder placed in a uniform magnetic field, and concludes by describing the phenomenon of induction, as shown in a Gramme dynamo. If a coil of wire be withdrawn from a magnetic field, a current is induced in the coil. A current of nearly the same strength is induced, if an iron cylinder be pushed over the coil. A coil of wire wound round an iron cylinder placed in a magnetic field will give an induced current, if displaced towards the periphery of the cylinder, and this current remains the same, if the cylinder be simultaneously caused to rotate about its axis. So in the Gramme dynamo, there is no current if the coils are fixed and the iron revolves, provided the residual magnetism of the iron can be neglected; but if the iron be fixed, or revolve with the coils, a current is set up; since the portion of the coils within the iron cuts a magnetic field much less intense than that cut by the outer portion.'

E. H.

On the Attractive Force of Ring-shaped Electro-Magnets.

By A. Wassmuth.

(Sitzungsberichte der Kaiserlichen Akademie der Wissenschaften, vol. lxxxv., February 1882, part ii., p. 327.)

The electro-magnets experimented on by the Author consisted of an iron core bent into a circular form and wrapped with several layers of wire. Magnets of this form were chosen to avoid any free magnetism, so that for small magnetising forces the magnetic moment could be measured with accuracy. The ring being bisected by a plane through its axis, the one half formed the magnet and the other the anchor. The latter was suspended to a balance by which the pull could be measured. The surfaces in contact were planed and polished, and means were provided to keep them exactly opposed. It may then be assumed that there is no free magnetism, and that the intensity of magnetisation is uniform over the surfaces in contact and the direction normal to them.

When these conditions are fulfilled the ordinary theory of magnetic induction gives the attractive force T in absolute measure by the equation,

$$T = 2 \pi q \mu^2,$$

where μ is the magnetic moment of unit volume, and q the sum of the areas of the opposed surfaces.

To determine μ in absolute measure a second wire was wound round the ring and connected to a mirror galvanometer. If the current in the primary coil be determined, and the constant and logarithmic decrement of the galvanometer known, the value of μ can be easily deduced from the throw of the mirror, when the primary circuit is broken. Taking a magnet of 81,800 milligrams weight, and with a mean radius of 58·4 millimetres, and with a circular section of 6 millimetres diameter, the Author obtained a series of values of T, corresponding to values of μ, from 532 to 13,075 in absolute measure.

Let a curve be constructed with μ for absciss and k for ordinate, where $\mu = k x$, x being the magnetic force; then for large values of μ the form of the curve approaches a straight line, which, when produced, cuts the axis of abscisses in a point $\mu = 14{,}000$. This is the maximum magnetisation obtainable, and is identical with Stefan's observations. To find the maximum of T a curve must be drawn having T for absciss and k for ordinate. This cuts the axis in a point T = 5·45, which is the maximum attractive force expressed in kilograms.

The quotient $\frac{T}{\mu^2}$, which from the theoretical considerations expressed in the above equation should be constant, diminishes rapidly as the magnetisation increases, but rises again for very large values of μ. The quotient $\frac{T}{\mu}$ is also not constant, but becomes a minimum somewhat before the coefficient of induction k attains its maximum. A series of experiments made with a second magnet of different dimensions again exhibited these characteristics very clearly, and they are also confirmed by some observations of Siemens.

The Author concludes the first portion of the Paper with a notice of the results obtained by separating the anchor from the magnet with a plate of non-magnetic matter. In the case of a glass plate the attractive force diminishes very rapidly as the thickness of the plate is increased; but if a very thin mica sheet be inserted, it was observed that for weak magnetisations the force was distinctly greater than when the metal of the magnet and anchor were in direct contact. At the same time the current induced in the secondary coil on breaking the current in the primary increased to the value corresponding to the same attractive force, when no plate was inserted.

The second part of the Paper is devoted to the consideration of a formula expressing the relation between T and μ more accurately than the one given above. In the latter formula the ring is conceived as made up of an infinite number of sections at an indefinitely small distance from each other, and the attraction of two such neighbouring surfaces is considered; but in actual experiment the extreme surfaces of the magnet and anchor do not actually touch but are at a finite distance; and hence it cannot be assumed that the influence of neighbouring layers can be neglected. The effect of this will in general be equivalent to an increase of distance, which explains why the quotient $\frac{T}{\mu^2}$ is smaller than $2\pi q$ from the point of inflexion of the curve, but does not explain the higher value of the quotient for smaller values of μ. The Author therefore assumes that the ring magnet consists of layers of finite thickness D, which increases to a maximum as the magnetisation increases. If d be the distance of two such layers, $D \times d$ is constant.

Starting from this assumption he shows that the force of attraction is expressed by an equation of the form,

$$\frac{T}{\mu^2} = \alpha + \beta\,\mu^2 + \frac{1}{\gamma + \delta\,\mu^2}.$$

Treating α, β, γ and δ as empirical constants this formula is found to be capable of expressing very correctly the values of T found in the two series of experiments above described.

E. H.

The Determination by Galvanometric Means of the Area inclosed by the Coils of a Bobbin, and on the Absolute Resistance of the Mercury-Unit. By Dr. F. Kohlrausch.

(Zeitschrift für Angewandte Elektricitätslehre, vol. iv., 1882, p. 542.)

The dynamic effect produced by a plane closed conductor is measured by the area of the surface enclosed by it; the geometrical determination of such an area from the length of the wire and the number of turns is not so simple as might be expected, owing to the pressure of the outer on the inner layers, the stretching of the wire during process of coiling, &c., and if the measurements are repeated by uncoiling the wire, this involves a destruction of the instrument for further research. The method, however, proposed by the Author is as simple as it is accurate, consisting as it does in the comparison of the effect produced by the bobbin, the dimensions of which are in question, with that of the single coil of a tangent galvanometer, the dimensions of which can be accurately ascertained by ordinary geometrical means. The same current is led through the bobbin and the galvanometer, the distance of the former from the needle of the latter being arranged so as to give a suitable deflection, and readings taken with the current of the same and opposite signs in the two.

Let i be the strength of the current, ϕ ϕ^1 the deflections with the same, and reverse currents respectively a the distance of the needle from the centre of the bobbin; the following are the equations obtained for the effect produced by the bobbin and tangent-coil which is balanced by the component of the earth's magnetism:—

$$\left(2\frac{F}{a^3} + \frac{2\pi}{R}\right) i = C \tan \phi;$$

$$\left(2\frac{F}{a^3} - \frac{2\pi}{R}\right) i = C \tan \phi^1.$$

R being the radius of the single coil, and F the surface, the value of which is required (the first and second members on the left-hand side of the equations represent the effect of the bobbin and coil respectively), whence

$$F = \frac{a^3 \pi}{R} \cdot \frac{\tan \phi + \tan \phi^1}{\tan \phi - \tan \phi^1},$$

ϕ^1 being, of course, negative if on the opposite side of zero to ϕ.

The equations given above maintain when the bobbin is placed (i) east or west of the needle and its axis passing through it; if placed (ii) north or south of the needle with its axis still east and west, the right-hand side must be multiplied by 2.

The above equations can be considered true only if the square of the ratio of the bobbin's dimensions to its distance may be neglected in comparison with unity; if otherwise, and neglecting under similar conditions, the sixth power of that quantity for $2\,\frac{\mathrm{F}}{a^3}$ must be inserted in (i) the expression—

$$2\,\frac{\mathrm{F}}{a^3}\left[1+\frac{1}{a^2}\left(\frac{1}{2}\,l^2-\frac{9}{10}\,\rho\right)+\frac{1}{a^4}\left(\frac{3}{16}\,l^4-\frac{9}{8}\,l^2\,\rho+\frac{45}{56}\,\rho^1\right)\right];$$

and in (ii) the expression

$$\frac{\mathrm{F}}{a^3}\left[1+\frac{1}{a^2}\left(-\frac{3}{8}\,l^2+\frac{27}{40}\,\rho\right)+\frac{1}{a^4}\left(\frac{15}{128}\,l^4-\frac{45}{64}\,l^3\,\rho+\frac{225}{448}\rho^1\right)\right],$$

where l is the length, r and r_1 the inner and outer semi-diameters of the cylindrical bobbin, and

$$\rho=\frac{r_1{}^5-r^5}{r_1{}^3-r^3}; \qquad \rho^1=\frac{r_1{}^7-r^7}{r_1{}^3-r^3}.$$

Errors from incorrect assumption of the centre of the bobbin can be eliminated by observations with the galvanometer on opposite sides of the needle, and taking for the true distance half that between the suspension of the needle in the two instances; the deflections should be reversed by a commutator, and the mean of the two readings taken, and also currents of different intensities employed; if the distance is so arranged that with reverse currents the effect on the needle is nearly nil, further precautions are not so necessary.

As a first trial of this method the Author has measured the bobbin, of electrical fame, used by Weber in 1853, and by himself in 1869 for determining the resistance in absolute measure of the mercury-unit. At that time he found the value of the absolute unit (10^9 C.G.S. units) to be about 2 per cent. greater than that obtained by the committee of the British Association, whereas the results obtained later by Lorenz F. Weber, Rowland, Rayleigh and Schuster, showed the true value to be somewhat smaller. Press of work prevented a repetition of the experiment hitherto; as, however, the dimensions of the bobbin was the only quantity not personally determined, the values given by Weber in 1853 having been accepted, a measurement by this method was in every way desirable. The results now obtained gave for the surface 387,200 square centimetres instead of 392,800, the value previously adopted, equivalent to a reduction of about 1 millimetre in the mean semi-diameter. As to the cause producing such an alteration, whether due to the pressure of the external layers or physical change after the lapse of so many years, no positive conclusions can be arrived at; supposing the change, to whatever cause due, to have occurred before the year 1869, when the instrument was already sixteen years old, the alteration in the Author's value of the absolute unit would be proportional to the squares of the figures given, and reduce the value of the mercury-unit to 0·944 ohm, making the B.A. unit 0·990 ohm, thus approximating to the results of the physicists already mentioned.

F. J.

A Form of Tangent-Galvanometer suitable for measuring powerful Currents. By the late F. J. Pirani, M.A.

(Transactions of the Royal Society of Victoria, vol. xviii., p. 6.)

This instrument, extremely simple in its construction, at the same time portable and accurate, consists of a band of copper doubled on itself, and then bent so as to form two concentric circles in one plane of slightly different diameters, through which a current will flow in opposite directions; suspended at their common centre is a magnetic needle, the deflection of which can be read off by an index traversing a divided circle, or by a mirror reflecting a beam of light on to a scale in the ordinary manner. The leading wires are twisted together to eliminate their effect on the needle. The Paper is illustrated by a diagram of the arrangement.

F. J.

On a Means of Increasing the Sensitiveness of Reflecting Galvanometers. By Marcel Deprez.

(La Lumière Électrique, vol. vii., 1882, pp. 76, 77.)

The Author criticises the method proposed by Professors Ayrton and Perry,[1] and thinks that it presents the objections of loss of light increasing rapidly with the angular amplification, and that the surfaces of the two mirrors must be accurately worked, for if irregularities were present, abnormal deviations of the luminous rays would be multiplied in the successive reflections, completely destroying proportionality.

In Deprez's method a plane mirror in the galvanometer receives the luminous ray and reflects it on to a cylindrical mirror whence it is reflected to the scale.

If a be the angle of incidence on the plane mirror, L the distance from the plane mirror to the surface of the cylindric mirror, and r the radius of curvature of the latter, the angle of the final ray with the incident first ray will be $2a\left(1+\frac{2L}{r}\right)$. As the deflection

---

[1] "La Lumière Électrique," 5th Oct. 1881.

in ordinary apparatus is equal $2\,\alpha$, the arrangement amplifies the actual indication in the ratio of $\left(1 + \frac{2\,L}{r}\right)$ to 1.

If, for example, $L = 1$ metre and $r = 0 \cdot 1$ metre the angular deflections will be multiplied twenty-one times, with insignificant loss of light.

The Author describes another method, purely mechanical, but of less general application. Suppose it is desired to measure the angle described by a moving body turning round a horizontal axis. Fix to this body a small spirit-level of which the radius of curvature is R; if the body turns on its axis through an angle α, the distance travelled by the bubble will be $R\alpha$, and the readings are the same as if made with an index of length equal to R. The precision that this method admits may be judged by remembering that the levels employed in geodesy have frequently a radius of curvature of 100 metres.

P. H.

On the Determination of the Ratio of the Electro-Magnetic and Electro-Static Units of Current-Intensity.

By Dr. IGNAZ KLEMENCIC.

(Carl's Repertorium für Experimental-Physik, vol. xviii., 1882, p. 505.)

The Author describes a new method for the determination of v, suggested to him by Boltzmann. Let a condenser be charged by a battery, and discharged n times in a second through a galvanometer. Then, if n be sufficiently large,

$$Q = n\,E\,K = C\,\alpha,$$

where Q is the quantity of electricity discharged; E the electromotive force of the battery; K the capacity of the condenser; C the constant of the galvanometer; and α the deflection. If a plate condenser be used, and f be the area of one of the plates, and δ their distance apart, then,

$$K = \frac{f}{4\,\pi\,\delta},$$

and

$$C\,\alpha = \frac{n\,E\,f}{4\,\pi\,\delta}.$$

To eliminate E the battery is connected with a galvanometer, whose constant C' is considerably greater than C, and the permanent deflection ϕ observed; then if W be the total resistance of the circuit,

$$C'\,\phi = \frac{E}{W},$$

and hence

$$W = \frac{C\,\alpha\,4\,\pi\,\delta}{C'\,\phi\,u\,f}.$$

Here W is measured in electro-static units; to convert it into electro-magnetic units the factor v^2 must be introduced, where v is the ratio of the electro-static to the electro-magnetic unit of current;[1] hence,

$$v = \sqrt{\frac{W \phi n r^2 C'}{4 a \delta C}},$$

where W is now measured in electro-magnetic units, and r is the radius of the plates of the condenser.

The Author arranged the apparatus in the following manner. The positive plate of the condenser was connected by wires to two mercury cups, A and C, and the negative plate put to earth.

The galvanometer had one terminal to earth, and the other to a cup B; and the battery its negative pole to earth, and its positive to a fourth cup D. Electrical connection was alternately made and broken between A and B, and C and D, by means of platinum wires attached to the ends of the prongs of a tuning-fork, and having their extremities dipping into the cups. By properly arranging the height of the mercury in the cups, the prongs on their inward excursion encountered C and D, thus charging the condenser, and on their retreat broke the connection between C and D, and made it between A and B, thus effecting the discharge of the condenser through the galvanometer. In this way the charge and discharge could be performed very rapidly, but at a rate which could be determined with great accuracy.

Two series of preliminary experiments were made to prove, first, that the deflection of the galvanometer was proportional to the electromotive force of the battery, and secondly, that the deflection was independent of the depths to which the wires dipped in the mercury cups.

The reduction factors C and C′, and ϕ the deflection corresponding to the electromotive force of the battery, were determined by passing known currents through different coils of the galvanometer. The needle of the galvanometer was rendered partially astatic, and had a free period of 14·7 seconds. The tuning-fork performed 126·16 vibrations in a second, and consequently discharged the condenser 63·08 times a second.

The subjoined Table gives the results of the experiments with different battery-power:—

	9 Daniells.	6 Daniells.	3 Daniells.
$v =$	29·98 10^{10}	30·34 10^{10}	30·38 10^{10}
		30·41	30·34
		30·48	30·70
			30·68

giving a mean value $v = 30{\cdot}41 \; 10^{10} \frac{\text{mm.}}{\text{sec.}}$

---

[1] The symbol v is the same as the "critical velocity" K of Clausius. Minutes of Proceedings Inst. C.E., vol. lxviii., p. 425.

This value lies between Maxwell's determination, 28·798, and Weber's, 31·07. If the influence of the rims of the condenser plates were taken into account, the result would approach more closely to the former value.

E. H.

The Measurement of Electrical Currents in Absolute Units.

(Zeitschrift für Angewandte Electricitätslehre, vol. iv., 1882, p. 363.)

Let a magnet, capable of turning in a horizontal plane, be suspended in the neighbourhood of a straight wire cutting that plane at right angles; if a current be sent through the wire, the needle will be deflected through a certain angle (a); and if the wire be considered as infinitely long, the following equation is obtained between the force exerted by the current and the horizontal component of the earth's magnetism, viz.:—

$$\frac{2\,M\,i}{d}\cos a = M\,H \sin a,$$

where i is the strength of the current, M that of the short magnetic needle, and d the distance of the wire from the magnet.

Now in the special case where $a = 45°$, the equation becomes $i = \frac{1}{2}\,H\,d$, and there is thus obtained the following simple rule.

To find the strength of an electric current in ampères, stretch a long wire, through which the current passes, vertically in a room, and bring a needle near it in the magnetic meridian; as soon as the needle shows a deflection of 45°, measure the distance of its deflected pole from the wire in centimetres, and this, multiplied by half the horizontal component of the earth's magnetism for the locality, gives the current in ampères.

The value of the horizontal component of the earth's magnetism may be obtained from the nearest Observatory, or by any of the usual methods.

F. J.

The Analytic determination of the best Elements of Construction of Carbon Transmitters. By GASTON BELLE.

(La Lumière Électrique, vol. vii., 1882, p. 158.)

The Author considers that inventors have proceeded only in a tentative manner, with loss to results attained. Some of the best conditions of a carbon transmitter are considered, and the following deductions are given. The sensitiveness of a microphone composed of several identical contacts, arranged in tension or in quantity, always remains equal to that of one of the primitive contacts taken alone, and therefore, by convenient grouping of the contacts of a microphone, its resistance may be varied without altering its sensitiveness. And all other things being under the most favourable conditions, the amplitude of undulation produced in a current by a microphone is in inverse ratio to the resistance of the microphone, and of the resistance of the battery, and proportional to the electromotive force.

P. H.

On the Electrical Resistance and the Coefficient of Expansion of Incandescent Platinum. By E. L. NICHOLS.

(Proceedings of the American Association for the Advancement of Science, 1881, p. 24.)

The Author gives an account of a series of experiments on the resistance and length of a platinum wire at various temperatures, ranging from 0° to the melting-point of the metal. The experiments were undertaken to afford data for comparing the existing formulas for deducing the temperature of platinum from its electrical resistance, with those by which the temperature is calculated from the coefficient of expansion.

A current from forty Bunsen cells was passed through a platinum wire 0·4 millimetre in diameter and 100 millimetres long. From two points of the wire, 55 millimetres apart and equidistant from the centre, leads were taken to a delicate galvanometer, by which the resistance of the wire between the two points could be accurately measured. Before and after each determination of the resistance, the length between the points was measured by means of microscopes provided with micrometer screws.

The following Table gives an abstract of the results from 0° to a temperature not far from the melting-point. The resistance and length of the wire at 0° are each taken as unity.

Resistance.	Length.	Resistance.	Length.
1·0000	1·00000	3·7090	1·01229
1·0410	1·00002	3·7813	1·01285
1·9000	1·00289	3·8904	1·01371
2·2934	1·00456	4·0303	1·01450
2·7821	1·00732	4·0655	1·01495
2·9696	1·00809	4·0841	1·01514
3·3741	1·01003	4·2005	1·01567
3·6449	1·01160	4·2447	1·01632

Dr. C. W. Siemens has published three formulas for the variation of the resistance of a platinum wire with the temperature.

$$\begin{aligned}
&(a)\quad r = 0{\cdot}039369\ \mathrm{T}^{\frac{1}{2}} + 0{\cdot}00216407\ \mathrm{T} - 0{\cdot}24127\\
&(b)\quad r = 0{\cdot}0021448\ \mathrm{T}^{\frac{1}{2}} + 0{\cdot}0024187\ \mathrm{T} + 0{\cdot}30425\\
&(c)\quad r = 0{\cdot}092183\ \mathrm{T}^{\frac{1}{2}} + 0{\cdot}00007781\ \mathrm{T} + 0{\cdot}50196
\end{aligned}$$

where T is the absolute temperature, and r the resistance. In (*b*) and (*c*) the temperature was measured directly by an air thermometer.

Benoit, for high temperatures, has given the formula—

$$(d) \quad r = 1 + 0{\cdot}002445\, t + 0{\cdot}000000572\, t^2;$$

where t is the temperature in degrees Centigrade.

Matthiesen, considering the change of length only, has found—

$$(e) \quad l = l_o\,(1 + 0{\cdot}00000851\, t + 0{\cdot}0000000035\, t^2),$$

and there is still the formula for the uncorrected platinum thermometer—

$$(f) \quad l = l_o\,(1 + 0{\cdot}00000886\, t).$$

In the subjoined Table the temperatures calculated from the above formulæ for given increases of resistance are compared with each other by help of the preceding results.

Length.	r.	Siemens.			Benoit.	Matthiesen.	Platinum Thermometer.
		a.	*b.*	*c.*	*d.*	*e.*	*f.*
		°	°	°	°	°	°
1·0000	1·00	0	0	0	0	0	0
1·0032	2·00	325	402	426	378	342	375
1·0082	3·00	692	812	1108	708	726	917
1·0146	4·00	1086	1244	1950	1000	1170	1323
1·0280	5·00	1464	1682	3170	1272	1638	3100
..	6·00	1828	2072	..	1512	2158	..
..	7·00	2170	2387	..	1766	2800	..
..	8·00	2470	2692	..	1978	..	..

These results show great divergences between the temperatures deduced by the several formulas. The accuracy of Benoit's formula (*d*) depends on the boiling-points of mercury, sulphur, cadmium, and zinc, and it has been shown by later researches that the values he used are not correct. The disparity between (*a*), (*b*), and (*c*) may be due in part to impurities in the metal, which, as Dr. Siemens pointed out, affect very considerably the law of change of resistance with the temperature. Hence it appears that in order to obtain trustworthy results from the platinum thermometer, each thermometer ought to be calibrated, so as to furnish a correct formula for the particular wire used, or great care should be taken to ensure that the wires used in different thermometers are identical chemically and physically.

E. H.

Electric Lighting of the "Comptoir d'Escompte" in Paris.
By A. H. NOAILLON.

(La Lumière Électrique, vol. vii., 1882, p. 199.)

This installation of the electric light possesses considerable interest, since it returns to the use of batteries, any engine being precluded by the authorities as unsafe and noisy. The battery employed is a form of bichromate element modified by Messrs. Grenet and Jarriant, and consists of zinc and carbon immersed in a bichromate solution, the zinc being made easily renewable, and capable of being raised from the solution when the battery is not in use. The exciting solution is supplied by a series of tubes from a tank, and there is an overflow to each cell. When the liquid is new it is supplied at the rate of 20 litres an hour to each battery, at the second passage at the rate of 30 litres, and at the rates of 40 and 60–80 for the third and fourth time of using, after which it is useless. Besides this constant renewal of solution, crystals are prevented forming on the carbon plates (of which there are four around the sides of the cell) by the injection of a current of air conveyed to the batteries by tubes, serving also to maintain constant agitation of the liquid. The liquid is formed of 1 part of bichromate of soda, 3 parts of sulphuric acid, and 10 parts of water. Each battery comprises forty-eight cells in tension, having an electromotive force of 82 volts, and giving a quantity of 24 ampères on short circuit, and will feed an arc lamp or eight to ten Swan lamps. There are to be sixty batteries when the installation is complete. A common return wire is used. The whole illumination includes fifty regulators of the Siemens form, and one hundred Swan lamps. Sixteen regulators are at present used in the great hall, and the lighting generally leaves little to be desired. The economy of the system has not yet been demonstrated, but its safety is acknowledged, and the estimate of cost is said to be capable of being reduced by recovery of the oxide of chromium from the exhausted liquid.

P. H.

Electric Lighting of Theatres. By AUG. GUEROT.

(La Lumière Électrique, vol. vii., 1882, pp. 126–129, 190, 191.)

This article is critical of the lighting by electricity of the Théâtre des Variétés in Paris. A first experiment was made in the lighting of the stage; eighty-three accumulators (each weighing 60 kilograms) furnished the current for eighty-three lamps, and the effect was good. At a second installation one hundred and ninety-one Swan lamps were lighted with one hundred and eighty-one accumulators of 60 kilograms and one hundred and thirteen of 40 kilograms. The Author then enters upon an examination on the data afforded by these trials as to the cost of lighting by incandescent lamps and accumulators. An advantage claimed for a system of lighting by accumulators is the lower motor power required. If, for example, there be needed for a given direct illumination 100 HP. during five hours, with an accumulator system only 25 HP. would be required, as it could be worked during twenty hours to charge the accumulators. In the majority of theatres, the lights are required from 7.30 or 8.0 P.M. till midnight, or if those lights required for the preparation of the stage, toilet of the actors, are counted, from 7 till midnight, or five hours' illumination each evening. To this must be added the lights required at rehearsal and for other purposes during the day, or a total of about two thousand hours of lighting a year. The Author then examines whether an accumulator will furnish light to a single lamp for five-and-a-half hours. Tresca's experiments showed that thirty-five elements, each weighing 43·7 kilograms including liquid, furnished an external work of 2,483 kilogrammetres per kilogram of weight, or, reduced to a standard of 60 kilograms an accumulator, 148,980 kilogrammetres an accumulator. Some measurements given in the "Electrician"[1] showed that an accumulator of 60 kilograms containing 81 lbs. of lead and minium furnished an external work of 189,100 kilogrammetres, or 5,147 kilogrammetres a kilogram. Professor Ayrton gives for this type of accumulator 198,000 kilogrammetres. These accumulators then give 10 kilogrammetres a second for five-and-a-half hours.

From the measurements made with Swan lamps it would appear that an intensity of thirty-two candles is obtained with an expenditure of 9·67 kilogrammetres. Each 60-kilogram accumulator corresponds therefore to a lamp of thirty-two candles burning during five-and-a-half hours, and the calculation of cost becomes easy; but the Author thinks that it would not be prudent to reckon that more than twenty candles are obtained for a work of 10 kilogrammetres per second, on account of practical losses. Assuming that 50 per cent. of the initial work appears in the circuit, two thousand accumulators would need for their charging 530 HP. to be effected in five-and-a-half hours, or 140 HP. for twenty-two hours' charging; but as this would need expensive night attendance, the Author assumes that 250 HP. for twelve hours would be the best arrangement, and, taking all items of coal, oil, depreciation, interest, and labour, into account, it appears that a lamp of twenty-candle power can be maintained alight for 0·0921 franc an hour on this large scale.

To this critical estimation Mr. Faure replies and the Author again answers. The reply refers to a reduced cost of accumulators and lamps; being 1,000 francs a tonne for the former, and an increased illumination for the latter, making the price of twenty candles' light an hour only 0·038 franc. The Author's answer is intended to maintain his figures as given.

P. H.

---

[1] 11 March, 1882.

The Origin of Incandescence Lamps. By C. de Changy.

(La Lumière Électrique, vol. vi., 1882, pp. 580–584.)

Although this is mainly a claim as to the invention of the electric incandescence lamp, it contains some particulars as to the Author's early experiments with incandescence in vacuo. In 1838, the Author claims to have published in the "Courrier Libéral," the idea of using a small carbon conductor in a vacuous chamber. In 1855 he abandoned carbon in favour of platinum, and his experiments led him to perceive that the metal should be prepared. This preparation was effected by slowly and gradually increasing the temperature to which the wire was raised, and by employing platinum submitted to an operation similar to the cementation of steel, in heating it in powdered charcoal, and afterwards drawing. Some lamps were thus constructed of considerable power and durability. In May 1858 the Author patented the following system of regulation. Each lamp was placed on a circuit derived from the general main, and besides the lamp the current traversed the wire of an electro-magnet; a second derived circuit branched from the first was formed by the core of the magnet and its armature; this second circuit was closed only if the electro-magnet put its armature in contact with the core. The current for safe incandescence was not sufficient to attract the armature against a spring. The action is to shunt an excess of current. In 1859, Count du Moncel, having described, in his researches on the Ruhmkorff coil, how he had obtained a very brilliant light by the incandescence of vegetable fibres, such as cork, bass, previously immersed in sulphuric acid and carbonised, the Author applied these to form the conductor of his vacuum lamp. He combined the uses of platinum and carbon in the same lamp, a bundle of carbons being surrounded by a spiral of metal. Lamps have been constructed of three to fifty candles by the Author.

P. H.

An Application of Electricity to the Transmission of Power at the Mines of La Péronnière.

By Messrs. Charousset and Bague.

(Bulletin de la Société de l'Industrie Minérale, vol. xi., 1882, p. 5.)

The energy in this case is transferred through conductors 1,200 metres (1,309 yards) long, from the mouth of the pit to the head of an incline 110 metres (120 yards) long, where it is employed to drive a windlass and draw the coal-tubs up the incline. There are two generating dynamoes and two receiving dynamoes, connected by the four conductors so as to form two independent systems and circuits, of which it is not generally found necessary to employ more than one at a time. The connection between the Gramme dynamoes and the driving machinery at one end, and between the dynamoes and the intermediate gear of the windlass at the other end, take place by means of friction-pinions made of compressed paper. The machines ride upon a centre situated below each of their octagonal frames, and are kept up to more or less intimate contact by screws in the case of the generators, and by a system of levers, under the immediate control of the attendant, in the case of the receiving machines, care being taken to provide against excess of pressure by a stop. As these machines when receiving a current always run in the same direction, whatever may be that of the current, the intermediate gearing contains a system of conical sliding-pinions for reversing the motion of the winding-drum.

The four machines employed are all octagonal and similar to one another. The ring is divided into four parts, passing successively before as many magnetic poles, alternately positive and negative. Each pole is formed by two coils whose cores are at right angles to each other, and have one pair of similar ends brought thus together.

The other ends of these eight cores are fixed in the octagonal frame, which therefore serves to connect opposite poles. Each of the four poles nearly completes the quadrant of a circle; and from the quadruple character of the magnetic field, four brushes are necessary to each machine. The coupling is in series—that is, the entire current flows through the electro-magnet coils of both the generating and the receiving machine.

The conducting cables are formed of sixteen copper wires of $\frac{11}{10}$ of a millimetre in diameter, the metal employed being perfectly pure. At the beginning of operations these wires, placed in juxtaposition, were first covered with a thickness of paraffined cotton, which again received two envelopes of cloth treated with Chatterton's compound, and was enclosed in india-rubber and tarred. But in spite of these precautions, the insulation did not remain perfect, on account of the damp situations in which parts of the cable had to be placed. A covering of lead was tried in damp places, but this turned out dangerous in practice, and the cables finally adopted were as follows:—

16 wires of copper $\frac{11}{10}$ diameter,
2 coats of paraffined cotton,
1 coat 5 millimetres thick of gutta-percha,
2 windings of Chatterton cloth,
1 thick coat of tarred cotton.

And the purchasers themselves finally covered the cables with a paste of the following composition:—

57 pints of Norwegian tar,
38 ,, resin,
5 ,, suet,

in order to protect them, as well from heated gases as from damp.

The original cable, still maintained in the dry parts of the mine, cost 1·25 franc per metre; that finally employed in the damp situations, 3 francs per metre.

The trouble which arose before the question of the cables was finally settled was instructive. When the cheaper cable was employed for the whole circuit, the insulation in damp situations became defective in about one month. Shocks were received by the miners even through the rock in the neighbourhood of the conductors, and these shocks finally caused the rupture of the cables themselves. The nature of these ruptures was peculiar. The ends of the wires exposed were untwisted, and the surface had lost its polish, being honeycombed with little cavities filled with a brown, or sometimes black dust. The paraffined cotton, which was very white when the cable was new, presented a green colour, and this discoloration extended to the exterior of the cable, and even to the walls of the pit. At four or five centimetres from the ends the wires were found to be intact. The oxide and carbonate of copper indicated a chemical action going on between the copper wires and the water in contact with the external envelope, under the influence of the electric current. It was on account of these ruptures that the lead covering was applied, but the safety of the cable was not found to be increased by this means. On one occasion the men below observed some large sparks in the pit, which greatly frightened them. The driver, warned by the galvanometer that something unusual was taking place, stopped the engine, and an investigation showed that the lead had been melted in several places, and thrown against the walls of the pit. Where the lead had been thus removed the Chatterton cloth was calcined, and the wires presented globules of metal which had been fused. All this occurred in a quarter of an hour, the receiving dynamo running free at the time. Since the application of gutta-percha to the cables, no such mishap has occurred. The following case of leakage occurred one day. The hauling cables, which are of iron wire, are attached to cast-iron projections on the drum, and by this means were in metallic connection with the bed-plate of the whole underground part of the installation. These cables were observed suddenly to be electrified, and at the same time a current passed through the telephone wire and set the bell ringing. Otherwise nothing abnormal took place in the running of the machinery. The phenomenon lasted two hours, during which it was impossible to touch the hauling cables without producing small disturbances, the effect increasing with the distance from the drum. On the bed-plate itself no shock was forthcoming.

The sudden disappearance of the phenomenon shows that it was due to some passing cause—perhaps to the presence of metallic dust on the india-rubber washers separating the bed-plate from the circuit traversed by the electric current; or, it may be, to the contact of one of the cables at a faulty point with the earth, for it was at that time that the cables began to show faulty insulation.

On a subsequent occasion, a fault in the bottom insulation of one of the machines certainly caused a derived current, and the hauling cable, then no longer attached to the metallic framework of the drum, was electrified more strongly than on the previous occasion.

It is very necessary to keep the nuts which secure the brushes firmly in the sleeve or holder quite tight. The neglect of this once caused the loosening of the brush, with the generation of large sparks.

The needle of the galvanometer indicated a much stronger current than usual, and the steam-engine was pulled up as if by a brake. The paper friction-gear became at the same time heated.

The following Table gives the usual performance of these machines. It is based upon an average of experiments. The galvanometer used is simply a needle, placed over the conducting cables. It will be observed that its indications are proportional to the squares of the difference of velocities.

—	Work Indicated in Engine-Cylinder per Minute.	Useful work in Coals raised per Minute.	Indications of Galvanometer.	Speed of Generating Dynamo. Revs. per Minute = V.	Speed of Receiving Dynamo. Revs. per Minute = v.	Difference of Speeds V − v.	Practical Efficiency.	Electrical Efficiency = $\frac{v}{V}$.
	Ft.-lbs.	Ft.-lbs.	°				Per Cent.	Per Cent.
1. Steam-engine quite unloaded	135,402	..	..	..	..	..	..	..
2. Steam-engine in gear . .	198,763	..	..	..	..	..	..	..
3. Steam-engine driving generating machine with circuit open	224,802	..	..	..	..	..	..	..
4. Receiving machine running unloaded	230,009	..	4·5	1,280	..	..	..	..
5. Windlass running, with no coal-tub	264,728	..	9·0	1,280	975	305	..	..
6. Windlass running, with one coal-tub	455,679	55,549	16·0	1,280	880	400	12·2	67·7
7. Windlass running, with two coal-tubs	526,776	96,778	19·5	1,280	830	450	18·6	64·8
8. Windlass running, with three coal-tubs	594,553	137,572	20·0	1,280	810	470	23·1	63·2
9. Windlass running, with four coal-tubs	663,989	173,592	25·5	1,280	780	500	26·1	61·0

T. H. B.

Experiments on the Transmission of Power by Electricity at the Mines of Blanzy. By — GRAILLOT.

(Bulletin de la Société de l'Industrie Minérale, vol. xi., 1882, p. 89.)

These experiments were instituted in order to arrive at a knowledge of the efficiency to be expected in transmitting the energy of a steam-engine above ground to machinery in the mine. The motor employed was a condensing Watt engine, without expansion, whose efficiency was assumed to be, during the experiment, constantly at 0·60. An intermediate shaft, with pulleys and belts, conveyed the motion of the engine to the generating dynamo.

The distance, 634 metres (= 691 yards), was represented by two coils of cable, each 604 metres long, and composed of nine copper wires $\frac{11}{10}$ millimetre in diameter, enveloped successively in cloth, in india-rubber, and in lead. The remaining 30 metres (33 yards) were made up by a brass wire 5 millimetres in diameter. The receiving dynamo actuated the winding machinery by frictional contact with the intermediate gearing. The course of investigation adopted was to make a series of observations, by cylinder-diagrams, of the power necessary to drive the intermediate gearing alone at different velocities. This, reduced in the proportion of 10 : 6, was in each case deducted from the power (reduced in an equal ratio) required in the various experiments with loads. The balance was taken to be the power transmitted to the generating dynamo, and this is finally compared with the useful work done by the windlass in weight raised.

The results obtained are open to the criticism, passed upon them indeed by Mr. Rossigneux, that they rest upon the assumption of the constancy of the efficiency co-efficient of the steam-engine under differing loads; but the results obtained are valuable, as showing the variation in the useful work when the other conditions remain fairly uniform, and as qualifying the dogma that as the work given out diminishes the efficiency increases. The length of the conductors within the limits (634 metres and 30 metres) of these experiments seemed to have no sensible influence upon the efficiency. When the work to do was small the efficiency was small, and rose as the work approached in value that for which the machinery was designed. Thus, with the long conductors, when the work utilised was 0·73 HP., the efficiency was 17·25 per cent.; at 1·15 HP. it varied between 26 and 34 per cent.; at 2 and 2½ HP., between 40 and 51 per cent.; and at 3½ and 4 HP., between 37½ and 51 per cent. With the short conductors, at 1½ HP. of useful work, the efficiency was 28 per cent.; and at 2 and 3 HP. it varied from 40 to 51½ per cent.

T. H. B.

Electrolysis in Dyeing and Printing. By Dr. F. GOPPELSRŒDER.

(Bulletin de la Société Industrielle de Mulhouse, 1882, p. 270.)

The Author has previously produced colouring matters by electrolysis. The following description, though in no way related directly to engineering subjects, would appear to indicate a probable means of multiplication of drawings. To produce aniline black on tissue or on paper, it is impregnated with an aqueous solution of an aniline salt, and then placed on a metal plate which is in connection with one of the poles of a small dynamo machine or battery. A second plate, bearing in relief the design to be copied, is placed on the tissue and connected to the other pole; or a pencil of carbon may be substituted for this second plate, and the design drawn or writings made. The design thus produced in black is indelible. Other colours can be printed.

P. H.

Amplitude of Telephonic Vibrations. By G. SALET.

(Comptes rendus de l'Académie des Sciences, Paris, vol. xcv., 1882, p. 178.)

By a method of displacement of Newtonian rings, the amplitude of the vibration of the plate of a receiving Bell telephone has been found to be 2 to 3 ten-thousandths of a millimetre. The transmitter was an ordinary Boettcher magnetic instrument.

P. H.

Magnetic Telephone with Concentric Poles.

By Dr. A. D'ARSONVAL.

(La Lumière Électrique, vol. vii., 1882, p. 150.)

Experience has shown that the principle of the Bell telephone should be preserved. But it is susceptible of some modifications. The Author early found in his trials that the best effects are produced when a flattened form is given to the magnet-poles, and these are separated by a minimum space; but this is not due to the increase of attractive force. By preserving in the magnet the same carrying-power, the Author has obtained less powerful effects with cylindrical poles than with flattened poles, the quantity of wire remaining the same. The most influential factor appears to be the length of wire comprised between the two poles, that is, the wire wound on the core in the magnetic field, where the lines of force have their maximum density. A simple experiment confirms this. The two poles of a magnet, distant only a few millimetres, are placed under a vibrating plate of soft iron, normally to the surface, and very close, as in a telephone. Under the same plate, by the side of the magnet, is stretched a wire traversed by an interrupted current. When this wire is placed in any other position than between the poles of a magnet, the diaphragm gives least sound. To obtain, therefore, a maximum effect, it is necessary to core[1] the wire completely in the magnetic field. The Author describes a telephone, giving excellent results, in which the wire is set in an annular magnetic field, as in a Nicklès' electro-magnet, taking for centre or core one of the poles of the magnet, whilst the other pole forms a circular envelope. The circular form of magnet, as used by Ladd in other electric instruments, by Niaudet in the Gramme dynamo, and by Ader in telephones, is adopted. With a telephone weighing 350 grams, speech is reproduced without alteration of timbre, and, mounted properly, can be heard through a room.

P. H.

Dynamo-Electric Brake or Dynamometer.

By Gustave Richard.

(La Lumière Électriqne, vol. vi., 1882, p. 561.)

This is a preliminary description of a dynamo-electric brake designed by Marcel Deprez; the Paper is otherwise descriptive of most of the known forms of mechanical dynamometers. The dynamo-electric brake consists of a Gramme dynamo, the base and fixed electro-magnets being mounted and swung on two knife-edges, which are set in the prolongation of the axis of the ring. This axis is set to revolve in journals independent of the rest of the machine. An index admits of measuring the angle of deflection of the electro-magnets and base as swung by the reaction of the magnetic field, developed by the inductors, on the ring; the whole arrangement thus constitutes an absorption-dynamometer.

If P be the weight necessary to be added to a balance-lever, attached to the axis of suspension of the induction system, to bring the index to zero; d the distance of the point of suspension of this weight from the axis of the ring, the work T developed per turn of the ring is given by $T = 2 \pi P d$. By the aid of this arrangement Mr. Deprez has verified the accuracy of the law expressed by the formula

$$T \frac{E I}{w g}$$

in which E is the electromotive force of the current generated by the machine; I the quantity; w the angular velocity of the ring; g acceleration of gravity.

P. H.

---

[1] To wind the wire around a core is termed by the Author "to core" a wire. —P. H.

Lightning-Conductors and Gas-Piping. By H. Aerts.

(Bulletin de l'Association des Gaziers belges, 1881–82, p. 35.)

1. In a building protected by a system of lightning conductors as usually erected, is it advisable to put the roof-conductors in connection with the gas-pipes?

2. May the gas-pipes be utilised as the sole lightning-conductor?

The records of accidents by lightning, and the opinion of most scientific men, including Mr. Melsens of Brussels, who has devoted much attention to the subject, concur in warranting a decided affirmative to the first question.

The second has never been definitely solved, and it is probable that, in the present state of scientific knowledge, an absolute rule could with difficulty be laid down. The author is, however, disposed to answer in the negative as far as most cases are concerned. The instruments now in use permit, indeed, of determining with great exactitude the resistance of a circuit to an electrical current of given intensity; but there can be no comparison between the current of a battery and the instantaneous lightning shock, which, moreover, varies greatly in intensity.

Taking, by way of hypothesis, the figure of 100 square feet, given by Mr. David Brooks, as the minimum area of metallic surface of a lightning conductor to be placed in contact with water, being in fact sufficient for a soil of mean conductibility, the gas-piping, which must be carefully connected with the conductor, should have a length of 8 feet 3 inches if of 2 feet diameter, and 46 feet if of 4 inches diameter.

When lead-joints are employed, the contact may be considered good, although possible derangements inducing a break of contact must be taken into consideration. With most arrangements of india-rubber joints there is no metallic contact; and, as india-rubber is essentially a bad conductor of electricity, special means for ensuring contact must be employed.

The preceding considerations appear to warrant the opinion that it is generally dangerous to utilise the gas-pipes of a building as the sole lightning-conductor; and that in all cases it is important to take special precautions according to the diameter of the pipes, the system of joint, and the nature of the soil. It is, besides, specially necessary to make several connections, some distance apart, between the conductor and the pipes; and it is advisable to precede any contact with the gas-pipes by contact with the water-pipes, or to interpose some metal plates or perforated pipes reaching down to the damp soil.

J. W. P.

On the Specific Heat of Strongly Magnetised Iron, and the Mechanical Equivalent of a Diminution of Magnetism by Heat.

By Anton Wassmuth.

(Sitzungsberichte der Kaiserlichen Akademie der Wissenschaften, May, 1882, p. 997.)

Stefan has proved that the specific heat of magnetised iron must be greater than that of unmagnetised iron (Sitzb. d. k. Akad, 64 Bd., p. 28). Consider a mass of soft iron brought into the neighbourhood of a permanent magnet, and then demagnetised by the absorption of a quantity of heat W_1. Let the iron, so heated, be then removed to an infinite distance from all magnetic matter without the expenditure of work, and deprived of a quantity of heat W_2, so that its temperature is reduced to the initial temperature (for example 0°); and let it then be again attracted by a magnet and magnetised. If W_1 were equal to W_2 work would be done in the above cycle without the expenditure of energy, hence $W_1 > W_2$.

Assume, for simplicity, that the iron is magnetised at 0°, the maximum moment at this temperature being denoted by m; and let a curve be drawn, having μ, the magnetic moment, for absciss, and κ, the coefficient of magnetisation, for ordinate. Then $\mu = \kappa x$, where x is the magnetic force; and the curve cuts the axis of abscisses in a point, whose absciss is m. If τ be the temperature to which the iron, when magnetised to saturation at a temperature of 0°, must be raised in order that its magnetism may completely disappear; and $c_{o\tau}$, the mean specific heat between 0° and $\tau°$ of the magnetised iron, and $c_{o\tau}$, the mean specific heat of unmagnetised iron, then,

$$W_1 = C_{o\tau} \cdot \tau ; \text{ and } W_2 = c_{o\tau} \cdot \tau \quad . \quad . \quad . \quad (1)$$

The energy acquired by the iron in moving from an infinite distance must be the equivalent of the heat $W_1 - W_2$; and, neglecting slight changes of temperature and volume, it must also be equal to A_0, the work done by the permanent magnets. Hence, provided the iron be in the form of a ring, so that there can be no free magnetism,

$$\left.\begin{aligned} A_0 &= \int_o^m x \, d\mu, \\ &= I \ (C_{o\tau} - c_{o\tau}) \ \tau \end{aligned}\right\} \quad . \quad . \quad . \quad . \quad (2)$$

when I is the mechanical equivalent of heat. This equation determines C, if τ be known. At a temperature somewhat above red heat (about 1,000°) it is not possible to magnetise iron with a moderately strong magnetising force, though it cannot be asserted that no force, however great, will magnetise the iron at this temperature; hence τ must at any rate be greater than 1,000°.

From the Author's own experiments it would appear that at a temperature of 500° the magnetic moment of iron is about 10·5 per cent. of its maximum.

Now suppose that the iron is magnetised to its maximum moment at a temperature t_1°, instead of 0°, and that the corresponding work done in magnetisation is A_1, then

$$A_1 = I\,(C_{t_1\tau} - c_{t_1\tau}) \cdot \tau_1 \quad . \quad . \quad . \quad . \quad (3)$$

when $\tau_1 = \tau - t_1$, and $C_{t_1\tau}$, $c_{t_1\tau}$ denote respectively the mean specific heats of magnetised and unmagnetised iron between the temperatures t_1 and τ. Similarly, for a temperature t_2, the equation is obtained,

$$A_2 = I\,(C_{t_2\tau} - c_{t_2\tau}) \cdot \tau_2 \quad . \quad . \quad . \quad . \quad . \quad (4)$$

The Author now assumes for the purposes of the argument, that the mean specific heats of magnetised and unmagnetised iron between the temperatures 0 to τ, t_1 to τ, and t_2 to τ may be taken as constant. Hence, denoting them by C and c, the above equations become

$$\left.\begin{aligned} A_0 &= I\,(C - c)\,\tau \\ A_1 &= I\,(C - c)\,\tau_1 \\ A_2 &= I\,(C - c)\cdot\tau_2 \end{aligned}\right\} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (5)$$

whence

$$\frac{A_0 - A_1}{A_0 - A_2} = \frac{t_1}{t_2} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (6)$$

If the relation between x and μ were known for all temperatures, the integrals A_0, A_1, A_2 could be at once evaluated, but for high temperatures this relation is not known; hence the work corresponding to several magnetic moments must be found and taken as abscisses of a curve, of which the ordinates are the corresponding values of κ. This being approximately a straight line when produced, will cut the axis in a point, the absciss of which is the integral sought. In this way the Author finds for two bars $A_0 = 162{,}500$, and $B_0 = 140{,}000$ respectively for the temperature of the room, taking the milligram and millimetre as units. The first bar, when raised to a temperature 529° gave $A_1 = 108{,}000$, and the second bar $B_1 = 105{,}000$ for 287°.

Hence

$$\begin{aligned} A_0 - A_1 = 54{,}500 &= I\,(C - c)\,t_1 \\ &= (C - c)\,4{,}155 \cdot 10^6 \cdot 529, \end{aligned}$$

and therefore

$$C - c = \frac{2\cdot48}{10^8}.$$

and

$$B_0 - B_1 = 35{,}000 = (C - c)\,4{,}155 \cdot 10^6 \cdot 287,$$

giving

$$C - c = \frac{2\cdot93}{10^8}.$$

Whence the mean value for the differences of the specific heats is $\frac{2\cdot7}{10^8}$.

It is now possible to determine the value of τ, the temperature at which no magnetising force however great will induce any magnetism whatever in the iron. Substituting in equations (5) the above values for $C - c$, the mean value of τ is 1,346°. This temperature is somewhat higher than the melting-point of iron.

It is possible that the phenomenon of the diminution in the maximum moment for increase of temperature may be explained by the heat giving rise to a transverse magnetisation, which, as Siemens has shown, diminishes the resultant magnetisation in the longitudinal direction; and it is known, both from theory and experiment, that pressure along the axis of magnetisation has a similar effect.

E. H.

On a Colorimetric Method of Determining Manganese.

By A. Ledebur.

(Berg- und hüttenmännische Zeitung, vol. xli., p. 417.)

The method described in this Paper was communicated to the Author by Mr. Goetz, of the Cleveland Iron Works, Ohio, where it is used in controlling the contents of manganese in certain varieties of cast steel; but it may also be applied to the examination of pig iron low in manganese (2 per cent. or below). Who originated the method is not known to the Author, he having been unable to find notices of it in print.[1] The quantity of material operated on is 0·2 gram, which is dissolved in nitric acid and diluted with water to the exact volume of 100 cubic centimetres. Ten cubic centimetres of the solution are then transferred to a beaker of about 60 cubic centimetres capacity, some nitric acid is added, and, when boiling, an excess of peroxide of lead. The coloured solution so obtained is filtered through asbestos, and transferred to a burette divided into tenths of cubic centimetres, the division reading from below upwards. The colour is then compared with that of a standard solution, made by dissolving 72 milligrams of crystallized permanganate of potash in 500 cubic centimetres of distilled water, 1 cubic centimetre of which will contain $\frac{1}{20}$ milligram of manganese. According to the depth of colour of the iron solution, from 1 to 4 cubic centimetres of standard solution are transferred to a second burette, of similar calibre to the first, and water is carefully added until both solutions are of the same tint when seen against a white-paper background. Then if *a* measures of standard solution require the addition of *b* measures of water to obtain uniformity of tint with the solution made from *c* measures

---

[1] The method was described as in use at the Gratz Steel Works. Minutes of Proceedings Inst. C.E., vol. lii., p. 359.

of metal, the proportion of manganese in the latter will be given by $\frac{a}{b} \cdot c \times 0 \cdot 25$ per cent.

The 10 cubic centimetres of the original iron solution contain 20 milligrams of iron, and each unit per cent. $\frac{2}{10}$ milligram of manganese, or four times as much as the standard solution. Each cubic centimetre of the latter will correspond to $\frac{1}{4}$ per cent. of manganese in the iron.

When the amount of manganese is very small, the operation may be conducted upon quantities of two or three times the normal volume, and conversely when it exceeds about 2 per cent. upon smaller volumes; but the latter method does not, as a rule, give satisfactory results. The following examples show the degree of accuracy obtained by the Author, with samples whose contents of manganese determined by analysis are given in the first column:—

	Mn.	I.	II.	III.	IV.	Mean.
1. Bessemer steel .	0·48	0·46	0·50	0·49	..	0·48
2. Pig iron . . .	0·77	0·86	0·88	0·70	0·72	0·79
3. Crucible steel .	0·14	0·10	0·11	0·14	..	0·12
4. Pig iron . . .	2·36	2·32	2·28	2·27	..	2·29
5. " . . .	5·68	5·32	5·33	4·69	..	5·11

The results are fairly accurate up to No. 4 inclusive, but it is evident that the method is not applicable to materials as rich in manganese as No. 5. For these the Author recommends Pattinson's volumetric method, as giving admirably accurate results after a little practice. Thus by it No. 5 was found by the Author to contain 5·68 per cent. of manganese, while the direct determination by weight gave 5·683 per cent.

H. B.

Transmission of Power by Vacuum.

By A. L. Petit and V. Tatin.

(Portefeuille économique des Machines, July, 1882, pp. 100–105.)

A fresh attempt is now being made in Paris, on a small experimental scale, to revive the old idea, traced back to Denys Papin[1] two hundred years ago, of transmitting power by vacuum. A line of wrought-iron tubes with india-rubber joints, laid in the sewers under two streets for rather more than one-third of a mile, connects an exhausting air-pump at one end with oscillating engines at the other, which are employed in driving lathes and various other small machines. The air-pump, driven by a belt, has a capacity of 1½ cubic foot, and maintains a vacuum of ¾ atmosphere, leaving

---

[1] *Acta Eruditorum, Lipsiæ*, 1688. See also Proceedings Inst. C.E., vol. xliii., pp. 53 *et seq.*, and pp. 161–2; and vol. xlix., pp. 21 and 37. Also Proceedings Institution of Mechanical Engineers, 1874, p. 202.

¼ atmosphere total pressure in the exhausted main. When driven as fast as 60 revolutions per minute, the rapid compression raises the temperature of the discharged air to 203° Fahr., or 126° above that of the external atmosphere, whereby the volume of the air discharged is increased about 38 per cent., thus marring the pump's efficiency. With the speed reduced one-half, to 30 revolutions per minute, the discharged air rises only 22° above the atmosphere, with only 4 per cent. consequent increase of volume. For transmitting 125 HP. it is calculated that an air-pump would be required of 63 inches diameter and 3 feet 7 inches stroke, running at 30 revolutions per minute. The diameter of the tubes should range from 2⅜ inches, up to a maximum of 6¼ inches for transmitting 1,000 HP.

Agreeably with theory, experiments show that the loss of useful effect in tubes transmitting power by vacuum is proportional to the length of tube exhausted, and to the square of the velocity of the air flowing through it, and inversely to the fourth power of the tube-diameter; so that the loss is represented by the expression $\frac{l\,v^2}{K\,d^4}$. By experiment, too, the coefficient K is found to be equal to 581, when l is in metres, v in metres per second, and d in centimetres; or taking l in yards, v in feet per second, and d in inches, K becomes 117,781. By the aid of this expression, the several conditions of any particular case may be so arranged that the loss in the tubes shall never exceed 2 or 3 per cent.

The oscillating engines driven by the vacuum are double-acting and expansive. Without expansion the power developed would theoretically be only 54 per cent. of that expended upon the exhausting air-pump; but with the most advantageous cut-off—namely at 3-8ths of the stroke, so as to expand 2⅔ times—the proportion would rise to 93 per cent. As the result of experiments, it is considered that a useful effect of 40 to 60 per cent. may practically be realised. By discharging the expanded air from the oscillating cylinder through a large and short port into a hollow pedestal of capacious cavity, to which the vacuum-main is connected, a prompt exhaust from the cylinder is ensured.

A. B.

The Sea-Cell as a Possible Source of Danger in Torpedo-Experiments. By H. Moors.

(Transactions of the Royal Society of Victoria, vol. xviii., 1882, p. 71.)

In considering the cause of a fatal accident, due to the explosion of a torpedo in an experiment conducted on board H.M.S. "Cerberus," the Author was led to the conclusion that though in this case it was "not proven," still the electrical action set up between the zinc plate of the torpedo case serving as earth for the firing-current, acting as one pole of a battery when immersed in the sea, and the iron hull of a ship with which the firing-line could accidentally make contact, acting as the other pole, might in certain cases be sufficient to ignite the fuze. A battery thus formed would have no resistance, and an electromotive force of about 0·514 to 0·564 volt; the platinum fuze ordinarily adopted has, according to the Table given in the "Chatham Instructions in Military Engineering," a resistance of 0·325 ohm cold, and 0·74 ohm at the fuzing-point, the current sufficient to fire a charge being 0·75 ampère; a more sensitive detonator has a resistance of about three times the former, and requires only 0·32 ampère; the conducting wire used in the experiment was such as to give 126·8 yards to one ohm. From these data it results that the total resistance in circuit for firing the ordinary fuze with such a sea-cell would be 0·752 ohm, and allowing for the increase of the fuze's resistance with temperature, it would certainly explode with 44½ yards of such a conductor in its circuit; and, owing to the uncertainty of the data adopted, a far greater margin of safety would be required. Experiments to test this theory were carried out by Messrs. G. S. Caldwell and G. Smibert, of the Post and Telegraph Department (the former was the first to propose the above theoretical deduction as the cause of the accident), and though unpublished, their results were placed at the Author's disposal. The experiments were made on board R.M.S. "Malwa"; a zinc plate, 7 × 3 feet, was lowered clear of the hull, and a small portion of the rail well cleaned, with which the conductor was put in contact; by this means eight fuzes of the regulation pattern were exploded through lengths of wire varying from 10 to 41 yards (175 yards of this conductor had a resistance of 1 ohm). An exposed surface of copper in connection with the iron, as was supposed to be the case on the "Malwa," would give a higher electromotive force than iron alone. The conclusions point convincingly to the possibility of accidental ignition from such a cause, and the necessity for due precautionary measures to prevent any such accidental contact in all torpedo experiments.

F. J.

Heating and Ventilating the Bourse in Berlin.

By Prof. H. Fischer.

(Zeitschrift des Vereines deutscher Ingenieure, vol. xxvi., 1882, p. 427.)

The Bourse in Berlin is being enlarged, and in connection with this, designs were procured for a complete system of heating and ventilation, which should apply to the old as well as the new portions of the building. The conditions specified to be fulfilled were, that with an outside temperature of − 20° Centigrade (− 4° Fahrenheit), after four hours' heating, the temperature 5 feet above the floor of the large halls should be 18° Centigrade (64·4° Fahrenheit); in the vestibules and staircases, 15° Centigrade

(59° Fahrenheit), and in the remaining rooms 20° Centigrade (68° Fahrenheit). The air in the vestibules and staircases must be changed twice an hour; in the large halls 12 cubic metres (424 cubic feet) of air must be supplied per hour per person, and in the other rooms 20 cubic metres (746 cubic feet). Twenty-four designs were received; that by Otto Meyer, of Peute, near Hamburg, secured the prize, and, with a few alterations, will be carried out.

The Author discusses the methods by which the various competitors solve the practical difficulties. In the best designs the air is slightly warmed at the point of admission to the building, and then distributed to local heating apparatus placed in or near the different rooms. Körting Brothers proposed in their design to effect the preliminary heating by passing the air, at a high velocity, through small pipes placed in a vessel of hot water. Messrs. Körting have found by experiment that the transmission of heat from water to air takes place under the above conditions at a much greater rate than by other methods. A sketch is given of this apparatus.

The prize design adopts a combined pressure and exhaust system, two sets of fans being used. The air is drawn into the building through a filter; it is then warmed by hot water coils to about 15° Centigrade (59° Fahrenheit), and is then forced through the supply air-passages, and passing over steam-pipes, enters the large halls about 9 feet above the floor; the exhaust is drawn off at the floor-level. Some of the offices in constant use have separate heating arrangements; these consist of ordinary hot-water coils, but the boilers supplying them are heated by steam-coils instead of fires; this enables the intermittent supply of heat from the main steam-boilers to be stored up sufficiently to keep the small hot-water coils continuously circulating.

The Paper is accompanied by plans of the buildings, and sketches of the noteworthy details of the prize and other designs.

W. P.

The Coefficient of Viscosity of Air. By W. Braun and A. Kurz.

(Carl's Repertorium für Experimental Physik, vol. xviii., 1882, p. 569.)

The Authors summarise the results obtained by previous experimenters using different methods for the determination of the coefficient of viscosity of air. Stokes has deduced from the experiments of Baily, with a cylindrical bar pendulum, the value 0·000104; while Meyer has found, from pendulum experiments made by various physicists, values ranging from 0·000184 to 0·000383. Maxwell, using Coulomb's method of the rotatory oscillations of disks, obtained the number 0·000200, and Meyer by the same method 0·000197. This method has the disadvantage of requiring a correction for the effect of the boundaries of the disk, hence the authors have used in place of a disk a sphere oscillating about an axis, thus avoiding the correction for sharp edges. If R be the radius of the sphere, K its moment of inertia, T the period of oscillation, μ the density of the air, and k the required coefficient, then the logarithmic decrement δ is obtained by the equation given by Kirchhoff

$$\delta = \frac{2 \pi R^4}{3 K} \sqrt{2 \pi k \mu T}.$$

A hollow ball, about 6 inches in diameter, and 0·19 inch thick, was hung by a bifilar suspension of cocoon fibres, 39 inches long, so as to have a period of complete oscillation about its vertical axis of 14·5 seconds. The value of K was determined by observing the change in period of oscillation, when a brass ring of known dimensions was attached to the ball, and μ deduced from barometric- and temperature-observations. Taking the mean of several values obtained for δ, the above formula gave k between 0·000175 and 0·000195.

E. H.

Notes on India-rubber. By C. Jenatzy.

(Bulletin de l'Association des Gaziers belges, 1881–82, p. 20.)

As the properties of great elasticity and tenacity coupled with perfect homogeneity and impermeability possessed by india-rubber, render it eminently suitable for purposes in which no substitute is yet found, it appeared desirable to conduct some experiments as to its elasticity and resistance to tensile strain.

The experiments were made on seven samples of the best Pará rubber (vulcanised by a special process with sulphide of antimony, and of 1·060 to 1·065 specific gravity) that maybe regarded as representing the best india-rubber used for industrial purposes. The samples were in the form of rings or washers of uniform rectangular section, and of sufficiently large diameter in proportion to the sectional area.

The various dimensions were carefully determined as far as possible by calculation, on account of the difficulty of measuring so soft and yielding a substance as india-rubber. Thus the external diameter was calculated from the circumference measured direct, and the internal diameter by deducting from it the breadth, measured by a pair of callipers correct to tenths of a millimetre. The volume was deduced from the loss of weight in water; and this figure, divided by the calculated mean circumference, gave the diametrical sectional area. Lastly, the height or thickness was ascertained by dividing the sectional area by the breadth, being also checked by direct measurement.

The rings were hung on a fixed hook, polished so as to avoid any chafing of the rubber; and from them were suspended a hook and plate, weighing together 1 kilogram (2·2 lbs.), the weight of the rubber being too slight to take into consideration. As gradually increasing weights were placed upon the plate, the ring first became elliptical, and then assumed an elongated form with straight and parallel sides. The elongations were measured between two lines previously made with ink, while the ring was held sideways between two straight-edges; and this marked portion was placed at the side so as not to be influenced by any rolling action over the hooks. As the suspended load was supported by the two sides equally, the results were recorded as if only half its weight was acting on a single band of india-rubber of the same sectional area as the ring.

The tabulated results of the experiments show that increments of elongation due to successive additions of equal weight increase up to a certain point, and then diminish gradually until the end of the experiment; and also that this point is nearly uniform for various samples. From this circumstance the author deduces the following law: "Under uniformly increasing loads, a band of india-rubber becomes elongated progressively up to double its length, from which point the elongations decrease." He also found that the weight required to quadruple the length of the band is three times that under which it becomes doubled.

J. W. P.

INDEX

TO THE

MINUTES OF PROCEEDINGS,

1882–83.—Part I.

LONDON:
PRINTED BY WILLIAM CLOWES AND SONS, LIMITED,
STAMFORD STREET AND CHARING CROSS.

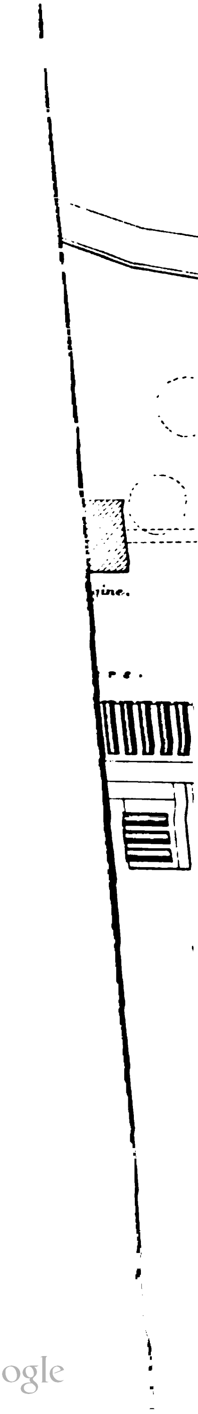
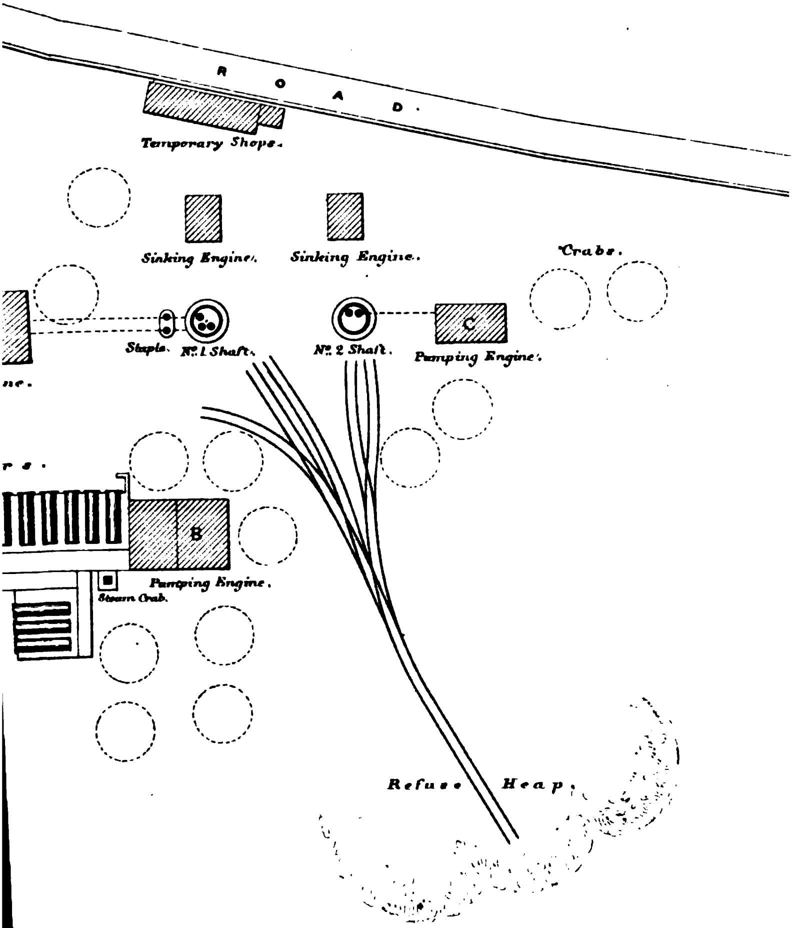


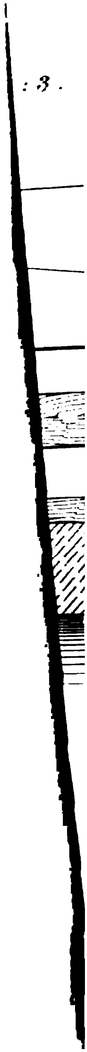
PLATE. 1.

Fig: 5.

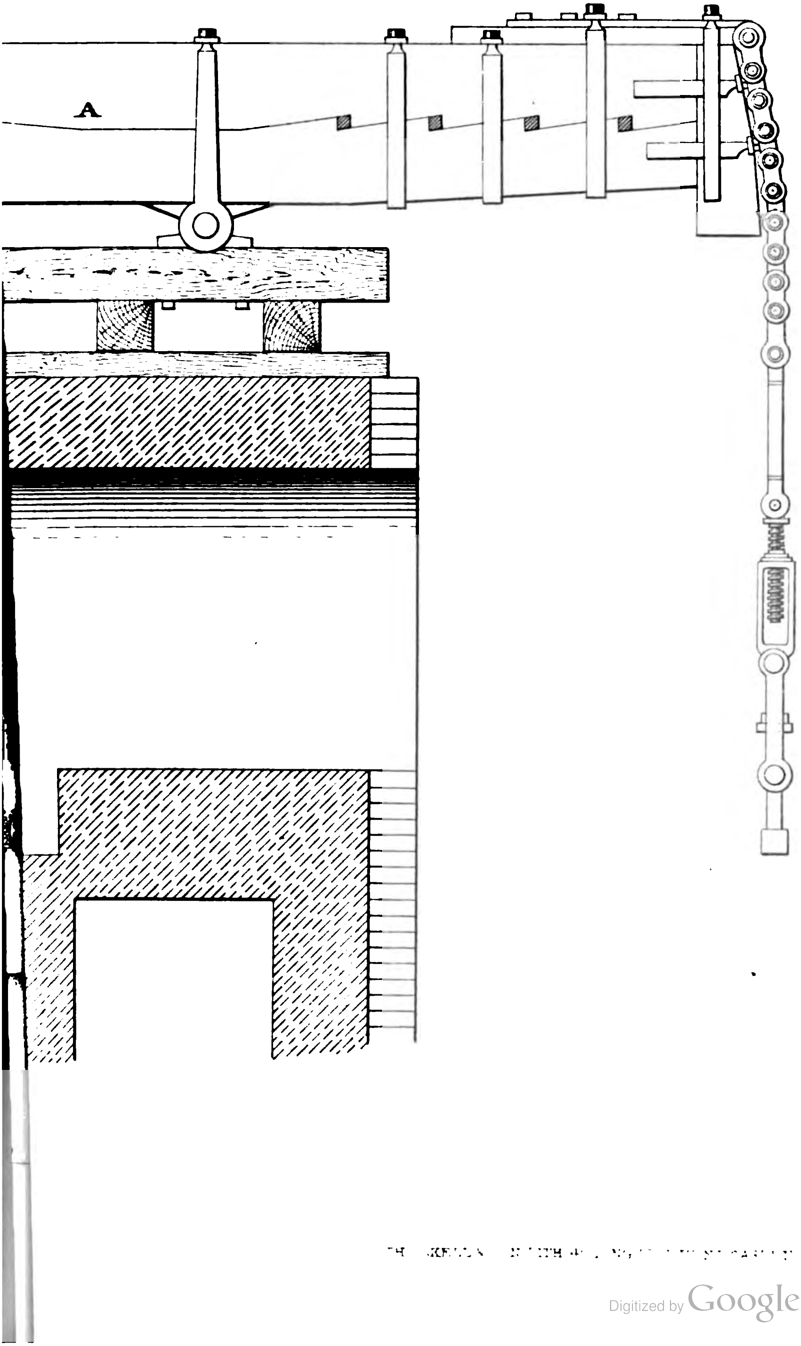


THOS KELL & SON, LITH. 40 KING ST COVENT GARDEN

: 3 .



: 3 .



[illegible]

B

PLATE. 3.

Fig: 15.

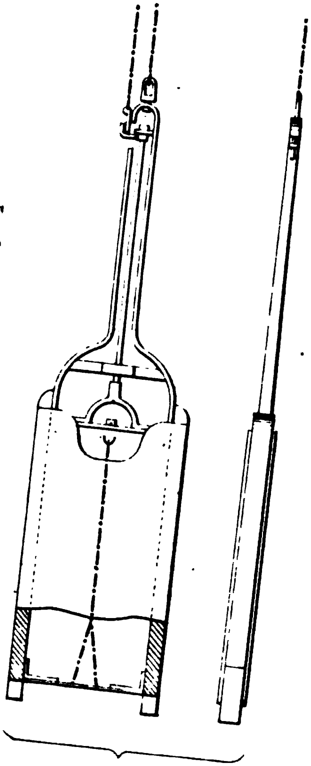


Fig: 16.

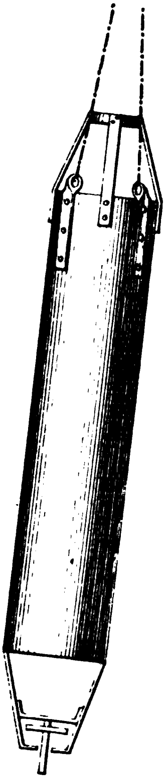
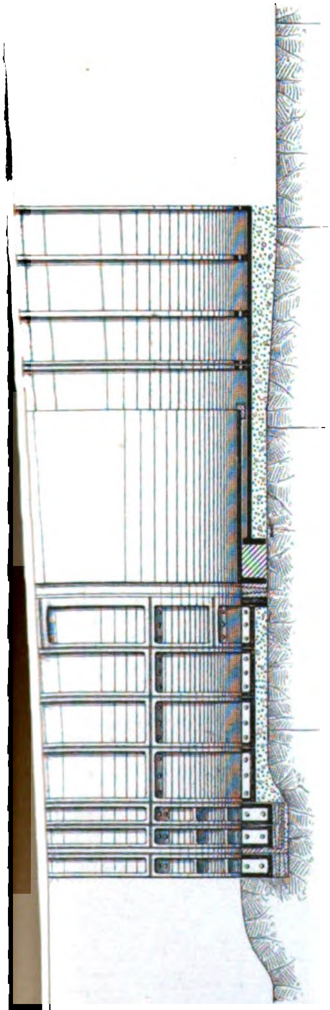


PLATE. 4.

7.

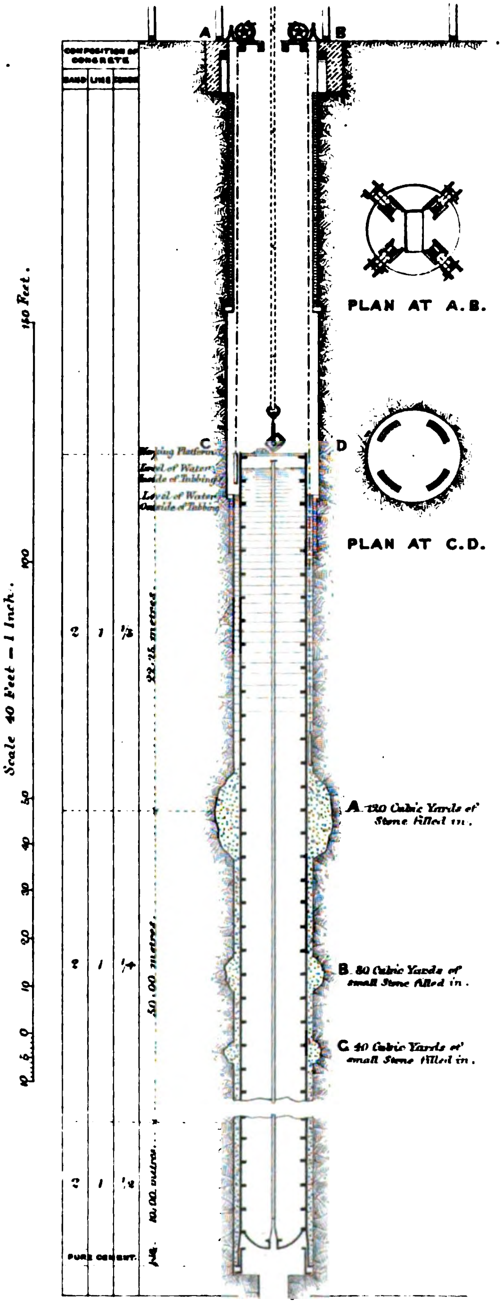


OX AFTER COMPRESSION,

d, and the Foundation Tubbing
g-Cribs in Place.

25 Feet.

Fig: 8.



SECTION OF Nº 2 SHAFT,

Shewing Position of Gullets and Composition of Concrete at different heights.

THOS KELL & SON LITH 4, KING ST COVENT GARDEN

STORA

PLATE. 5.

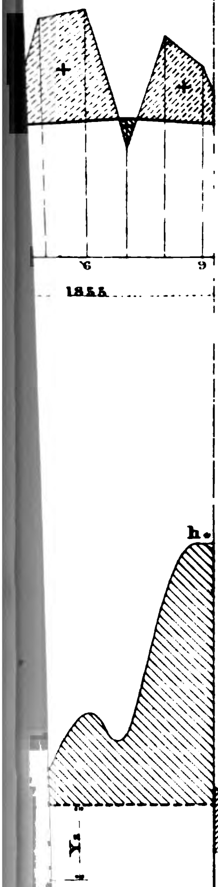
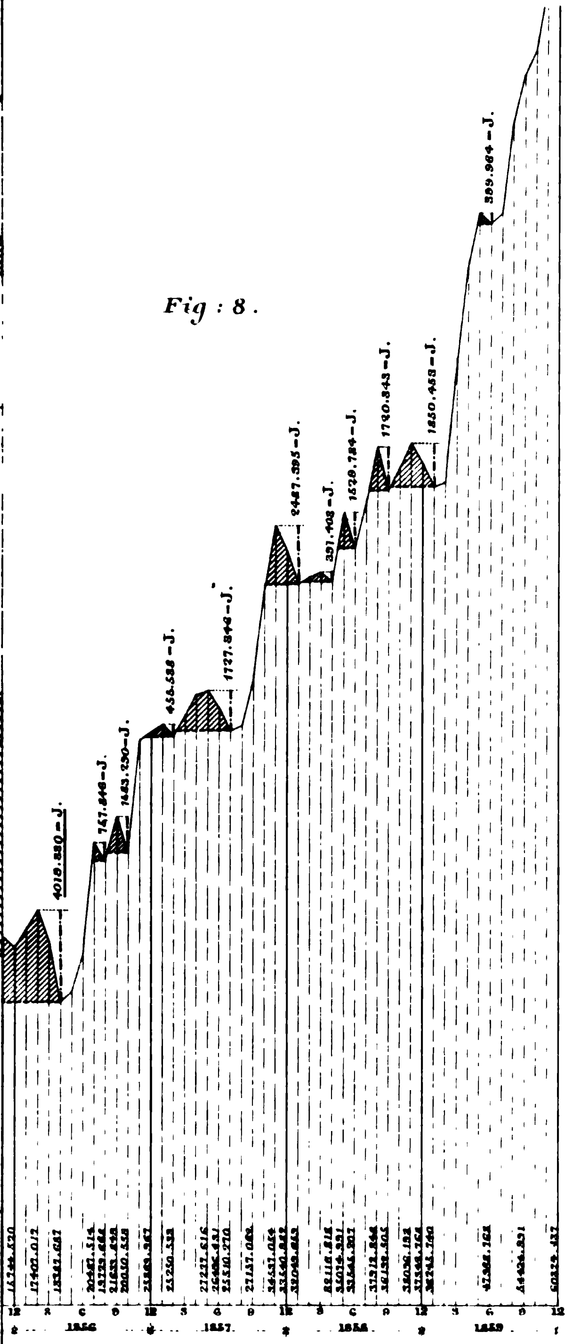
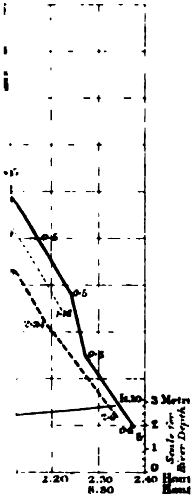


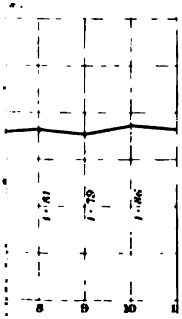
Fig : 8.



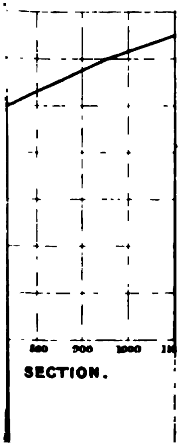
...of The Institut...

THOs KELL & SON LITH 40 KING ST COVENT GARDEN





T ONE PLACE.



SECTION.

Proc. Inst. Civil Eng.

V. 71 p. 270

TA
1
.I6m

[illegible] of Reffle

[illegible] Wed

10 AM

V. 71 = 1882/83 pt 1

MISSING	BINDERY	HOME CHARGE	AND CALL NUMBER IN PUBLIC CATALOG	IN BUILDING	CARRELL

FOR USE IN LIBRARY

Recharge This Book Before Taking Home

Author ....

Title ....

.... Date ....

Signature ....

Address .... Phone ....

VOLUME

BOOKS TAKEN FROM THE BUILDING ON THIS CHARGE ARE
SUBJECT TO A FINE OF 25 CENTS A DAY.
U. OF M. GENERAL LIBRARY

Form 6068